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CONTENTS.

PAGE.
The Determination of the Wave Form of Alternating Currents without a Contact Maker. (Illustrated.) By Harris J. Ryan. June 26th, 1899 365
Elements of Design Favorable to Speed Regulation in Plants Driven by Water Power. (Illustrated.) By Allan V. Garratt. June 27th, 1899 381
The Protection of Secondary Circuits from Fire Risks. By Cary T. Hutchinson. June 27th, 1899 427
Plea for a Working Standard of Light. By S. Everett Doane. June 28th, 1899 451
Motor Speed Regulation. By Alton D. Adams. June 28th, 1899 463
Proposals for Associate Membership 467
Advertisements 468

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A paper presented at the 16th General Meeting of the American Institute of Electrical Engineers, Boston, June 26th, 1899, President Kennelly in the Chair.

THE DETERMINATION OF THE WAVE FORM OF ALTERNATING CURRENTS WITHOUT A CONTACT MAKER.

BY HARRIS J. RYAN.

A method is given in this paper for determining the wave forms of alternating currents which uses no highly specialized measuring apparatus. No contact maker is used, and it is generally appliable on polyphase circuits only. It is recommended as a method which may be used under many circumstances where the special contact maker apparatus is not available.

Description of the Method.—In the diagram of Fig. 1, T is a special transformer with a closed magnetic circuit of soft, pure iron or "electrical steel" with the portion enveloped by the primary and secondary coils much restricted in area as compared with the remaining portion of the circuit. The circuit through each sheet is unbroken by joints, making it necessary to wind the secondary and primary by hand after the core forming the magnetic circuit has been built up. The primary coil is so proportioned that sufficient alternating current may be circulated in it to provide a maximum M. M. F. of about 500 C. G. S. per centimetre along the core at its restricted portion. Such exciting current may be taken from the secondary of an induction motor appropriated for the purpose, or from a set of phase changing transformers. Under these circumstances the flux through the core of this transformer will cause saturation during 97 per cent. of the time. The greater portion of the reversal of the flux will occur during 1.5 per cent. of the time of one alteration. The greater portion, therefore, of the secondary E.

M. F. time integral will occur during 1.5 per cent. of the time of one alternation and at the instant when the exciting current passes through zero. The remaining portion of the secondary time integral of the E. M. F. will be applied with symmetry throughout 98.5 per cent. of the alteration, being maximum on either side of zero for the exciting current, and gradually diminishing to zero during the time in which the exciting current is attaining its maximum value. It is seen then that the main portion of the secondary E. M. F. will be delivered with great suddenness at the time when the exciting current passes through zero.

In dealing with 40 cycles per second this E. M. F. will be delivered during 1.5 per cent. of the time of one alternation or in about 1/5000 of a second. It is seen also that the residue of the secondary E.M.F. constitutes an alternating pressure that is one-quarter cycle behind the primary exciting current. The cross-section of the restricted area of the magnetic circuit with reference to the amount of pressure applied in the primary circuit for excitation is proportioned so that the amount of irregularly delivered flux can not appreciably alter the exciting current, as it would otherwise be established from instant to instant.

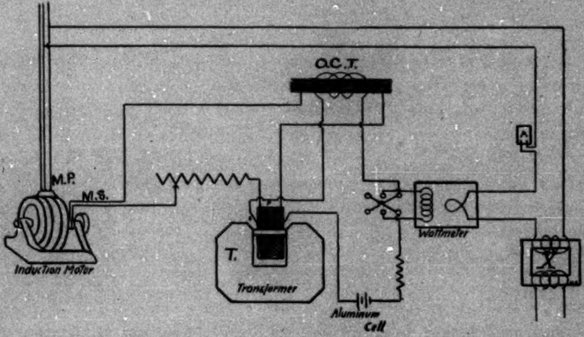

FIG. 1.

The secondary of the transformer T is connected to the pressure circuit of a dynamometer—a Weston wattmeter does very well—through the field coil of which passes the current whose form is to be determined and which is set up by the source pressure through some appliance X. Let us call the sudden time integral value of the secondary E. M. F. e, the instantaneous value of the current c, the effective value of the residue of the secondary E. M. F. E and the effective value of the current C, then the reading of the wattmeter will be:

$W = e\,c + E\,C \cos\theta$, where θ is the angle of phase difference between E and C, from which we may deduce the value of c. The phase of the exciting pressure delivered by M. S. may be changed by moving the motor secondary, or by a proper change in the value of the combined polyphase E. M. F.'s applied in the phase changing transformers, as the case may be, and a new instantaneous value of the current corresponding to the above change in phase will be obtained. In the same manner other values of the current may be obtained until the form of the current curve is sufficiently well defined. To apply the correction $E\,C \cos\theta$ involves so much attention and labor, as to make this method valueless if it were not for the fact that it can be quite completely compensated for in the following manner: The exciting current through the primary of the transformer T is passed through the primary of a suitably designed open circuit transformer O C T, and through the secondary of O C T the connection of the impulse secondary from T is made on its way to

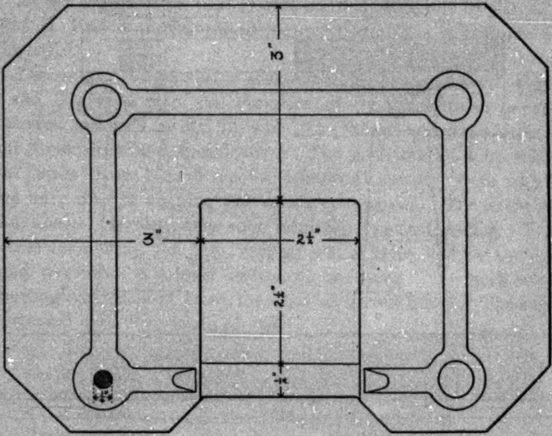

FIG. 2.

the wattmeter. The E. M. F. developed in the secondary of O C T will be one quarter cycle behind the primary exciting current of the impulse transformer T and therefore in unison with the residue E. M. F. of the impulse secondary of T. The secondary turns on O C T are adjusted in number so that the E. M. F. generated will equal the residue E. M. F. of the secondary of the impulse transformer T, so that when the connection through the secondary coil of O C T is reversed the residue E. M. F. of the T secondary will have been compensated for.

The reading of the wattmeter will then become $W = c\,e$, and since e is constant, the values of W will be proportional to c or the instantaneous values of the current.

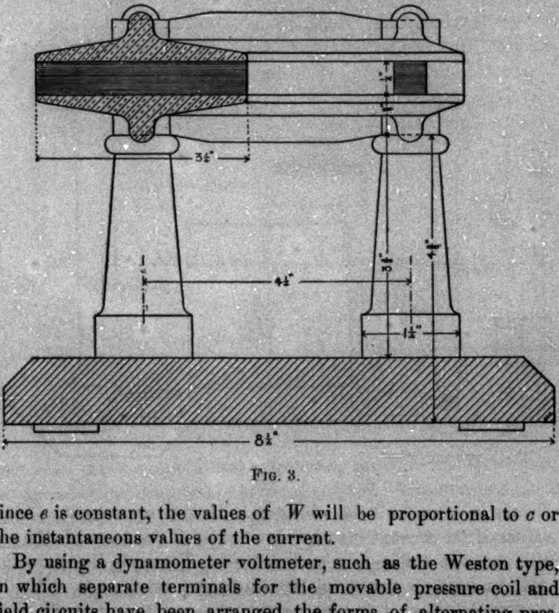

FIG. 3.

By using a dynamometer voltmeter, such as the Weston type, in which separate terminals for the movable pressure coil and field circuits have been arranged, the forms of alternating pressures and of tiny alternating currents may also be determined.

Thus this method is practicable only for instances where the plus and minus alternations are alike, which is the case in much alternate current practice. Where, however, the positive and negative forms differ, one or more aluminium cells should be placed in series with the impulse secondary. Such cells have small internal resistance and after having been charged with a direct current will completely cut off one side of the alternating impulse, provided the instantaneous pressure in the circuit at no time exceeds 20 volts per cell. Thus equipped the method may be used with Weston or similar make of instruments for form measurements in great variety and with ease and facility. This method is practically the same as Dr. Duncan's[1] except for the manner in which cyclic impulses are obtained.

A TRIAL OF THE METHOD.

At my suggestion a trial of this method was made by Mr. Lyman H. Brown during the spring of 1898 in the laboratories at Cornell.[2]

Design and Construction of the Impulse Transformer.—Figs. 2 and 3 give a plan and elevation of the transformer as constructed and used by Mr. Brown. The following are the important dimensions and specifications: The core was built up without joints from 10 mil Apollo "electrical sheets," from which the burr due to shaping was carefully removed. The oxide on the sheets afforded the only insulation against eddies. The frame for bolting the core together was of brass, and did not extend over the restricted section of the core. The bolts were thoroughly insulated from the core and brass frame. The supports and base were made of oak.

No. of turns in primary	547
Size of primary	14 B. & S. G.
No. of secondary turns adjustable	
No. of sheets	48
Thickness of a sheet	.01 in.
Area of contracted cross-section of core,	.25 sq. in.
Area of un-contracted cross-section of core,	1.5 sq. in.
Length of contracted section,	2.5 in.
Total length of magnetic circuit,	10 in.
Normal primary exciting current,	3 amperes.

On the assumption that a sufficient phase changing pressure is applied in the circuit, through the primary of this impulse transformer, so that the irregularities in the flux set up in the core will not appreciably alter the form of the exciting current, and with the aid of a B-H curve for the Apollo iron, I have calculated the following sequence of E. M. F's. that will be set up per turn throughout one quarter cycle in the secondary of this impulse transformer, when working at 40 cycles per second. The fractions in the time column give the portion of a quarter cycle that has lapsed since the exciting current passed through zero; the figures under B show the flux density in lines per sq. cm. in the contracted section of the core, and the values under E in the brackets, showing the time interval over which they were estimated, give the average of the E. M. F's. developed per volt in the impulse secondary.

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[1] TRANSACTIONS, vol. ix, p. 179.

[2] Thesis, Lyman H. Brown, Cornell University Library, T-1898, No. 30.

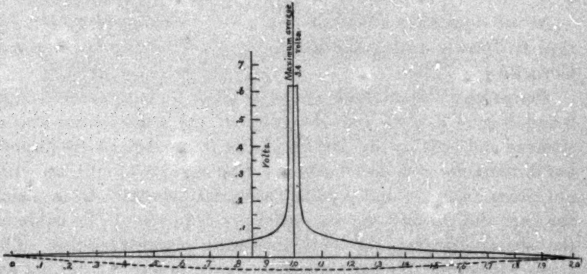

FIG. 4.

Time	B	E
0.00	0	3.24
.01	12,500	
.02	14,900	.623
.03	15,800	.1065
.04	16,400	
.05	16,800	
.06	17,200	
.07	17,400	
.08	17,700	
.09	18,000	
.10	18,200	.0644
.20	19,200	
.30	19,800	.0258
.40	20,300	
.50	20,800	
.60	21,150	.00644
.70	21,400	
.80	21,450	
.90	21,500	
1.00	21,550	

The corresponding impulse curve of E. M. F. and time, is given in Fig. 4 for a complete alternation. The broken curve shows the estimated compensating E. M. F. that should be provided by the secondary of the transformer O C T in Fig. 1.

Experimental trial made by Mr. Brown:—A compensating open coil transformer was used that had the following dimensions:—

Core of laminated iron, ring pattern	
Mean diameter of ring	8.75″
Cross-section,	$1\frac{7}{8}'' \times 2\frac{1}{2}''$
Air gap	$\frac{5}{16}''$
No. of primary turns of No. 10 B. & S. G.,	121
No. of secondary turns adjustable,	

A 150-watt scale Weston wattmeter, wound for 150 volts and two amperes, was used as a dynamometer; the resistance of the pressure coil was cut down to a point where the instrument operated over a desirable range. Ten secondary turns were placed on the impulse transformer, T, and the same number was found necessary for compensation on the σ O T transformer.

At x in Fig. 1, the secondary of the 10-light transformer reported upon in the TRANSACTIONS, vol. vii, p. 1., was connected through the field coil of the wattmeter, to one pressure of the triphase source. The primary was kept on open circuit. By means of an auto-converter, the source pressure was so adjusted that a normal flux was established in the core of the 10-light transformer, x. For a phase changer, a Siemens and Halske 3 H. P., 33-cycle, triphase, 4-pole, 110-volt induction motor with a ratio of transformation of approximately one, was used. By means of an index wheel and brake attached to the motor pulley, the motor secondary could be set to develop any desired phase for the exciting current of the impulse transformer. The phase changing pressure was set at 100 volts, and the current controlled partly by reactance, and adjusted to the desired value of approximately three amperes, by means of a

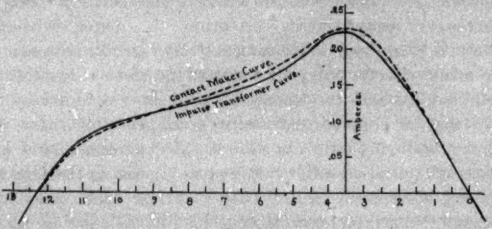

FIG. 5.

non-induction resistance. A complete reactance control and a higher phase changing source pressure would, however, have been better.

In Fig. 5, the unbroken curve is an alternation of the magnetizing current for the transformer at x, as obtained by Mr. Brown in the employment of this method, while the curve is the mean of the two alternations for the one given in TRANSACTIONS, vol. vii, p. 12, as obtained by the contact-maker therein employed. The three-phase generator which supplied current for the experiments, had its armature conductors distributed on a smooth core, and developed at no load a fair approximation to a sine curve of pressure. The full load current output of this generator was 20 amperes, while the induction motor used for phase changing, required 10 amperes for field excitation. The armature reaction produced by this exciting current, was comparatively large, causing the generated pressure curve to differ some from the pressure curve used at the time the original contact-maker measurements were made.

There is, therefore, a fair practical agreement between the curve taken with the polyphase impulse transformer method, and the curve taken with the contact maker. It was found by Mr. Brown that 10 impulse-residue E. M. F. conpensating turns were necessary, while my estimates showed that but five should have been necessary. This difference is probably due to the fact that the sheets were not annealed after they had been tooled into shape, and that complete reactance control of the exciting current for the impulse transformer was not used.

No experiments have been made in which the aluminium cell was used to cut out either the plus or minus impulses so that the forms of unsymmetrical curves could be determined. Experimental data have been obtained by H. J. Hotchkiss[1], from which it is seen that the aluminium cell will be found to be reliable for this purpose.

Department of Electrical Engineering,
Sibley College. Cornell University.
June, 1899.

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[1] *Physical Review*, March, 1899.

ADDENDUM

[COMMUNICATED AFTER ADJOURNMENT BY HARRIS J. RYAN AND E. F. SCATTERGOOD.]

Since the date at which this paper was read we have made with the aid of a carefully adjusted contact-maker a study of the impulse transformer, the method employed for compensating the impulse residue E. M. F. and the use of the aluminium cell to cut out alternate impulses.

We have found that the transformer employed in the trial made by Mr. Brown is not proportioned so as to give the best results. The saturated portion of the core is too long, the iron sheets are too thick and it is desirable to shorten the length of the induction path through the unsaturated portion of the core as much as possible. To avoid eddy currents as much as practicable, better insulation between the iron plates should be used than that provided in the transformer made by Mr. Brown under the direction of the author of this paper.

We have found that induction reactance, ($L\,\omega$), with the minimum resistance possessed necessarily by the circuit, should be used to control the current in the primary of the impulse transformer. Excellent results as will be seen below when the transformer is well constructed, are obtained by impressing upon the primary circuit an effective pressure that is double the maximum impulse volts which are to be delivered in the secondary where the ratio of transformation is one to one. A higher pressure would slightly elevate the impulse E. M. F. and improve it in form without changing its time integral. A lower pressure should not be used. In all cases the primary exciting current should be large enough to give the restricted portion of the core a maximum M. M. F. of 500 to 600 ampere-turns per .393 inches of length.

Below are given the specifications for an impulse transformer that we constructed to use in connection with our Weston or Thomson wattmeters. For small currents up to .05 ampere we have used the ordinary Weston alternating voltmeter, having separated its field and moving coil circuits and having drawn a watt scale underneath the voltmeter scale. Such a watt scale is quickly calculated from the voltmeter scale and the resistance of the instrument.

Number of sheets in core	100.
Thickness of each sheet	.006 in.

Length of saturated portion of core	.395 in.
Average length of unsaturated portion of core	7. in.
Ratio of width of saturated to unsaturated portion of core	3.9
Number of primary turns	20.
Size of primary conductor, B. & S. G.,	12.
Number of secondary turns	20.
Size of secondary conductor, B. & S. G.,	24.

Additional Specifications :—

Each core sheet was cut from photographer's ferrotype plate. This material was selected for the core because we could not wait for the arrival of thin Apollo "electrical steel" or material of equal grade. The ferrotype sheets, as is well known, are enameled making it unnecessary to provide further insulation to break up eddies. Each core sheet was cut to the following dimensions :

Size of sheet	4.5 x 5. in.
Thickness of iron	.006 in.
Thickness of sheet including enamel	.015 in.

Two openings to provide space for the primary and secondary coils were cut at the center of each sheet allowing a tongue, the saturated portion, to stand.

Length of tongue	.393 in.
Width	.4 in.
Size of each of these openings, length	.393 in.
width	.6 in.

The transformer was immersed in oil so that the primary coil would not become too warm.

The induction path in each sheet was continuous. Joints were not used to facilitate winding ; they would interfere with the desirable properties of the induction circuit. The primary and secondary coils were wound into position by poking each turn through the openings provided in the core.

In the above design we wished to make the average length of the unsaturated portion of the core quite as short as is practicable in an actual construction. From the performance of this and other impulse transformers that we have constructed and tested, we have found that the openings through the plates to accommodate the primary and secondary coils may be made broad enough to permit of the use of sufficient copper required to avoid undue heating when the transformer is operated in the open air.

The most available length of saturated core for use with Weston and Thomson instruments of the dynamometer type appears to be about .4 inch.

At 33.5 cycles this transformer gave an open circuit secondary impulse E. M. F. as follows:

Maximum value of impulse E. M. F., volts	24.
Width of impulse at top in per cent. of one half cycle	1.67 %
Width, one volt from base of impulse E. M. F.	5.55 %
Width, average of impulse E. M. F.	3.66 %

At 100 cycles the impulse E.M.F. had the following dimensions:

Maximum value, volts	56.4
Width, average, in per cent. of one half cycle	4.4 %

The forms of a number of impulse E.M.F.S were taken with the impulse secondary on open circuit and then closed through 50 non-inductive ohms and at other times through the moving coil of a Weston alternating voltmeter which had a resistance of 45 ohms, to determine the effect upon the form of the impulse E.M.F. caused by the draught of about that much impulse current which is drawn in making instantaneous measurements. The effect of the draught of this current was noticed in all cases. It lowered the impulse E. M. F. maximum by approximately one tenth, and increased the average width about the same amount.

Under all circumstances this transformer is used with a primary exciting current of 20 effective amperes. In series with the primary is inserted a self-induction of .0095 henry. Forty effective volts are, therefore, required for primary excitation at 33.5 cycles and 120 volts at 100 cycles.

The following method for determining the number of compensating turns to be used as described in the paper in connection with O. C. T. in Fig. 1 was found to be entirely reliable. These opposing secondary turns at O. C. T. are adjusted in number by trial so that the wattmeter in Fig. 1 reads one-third less with compensating turns than it does without them.

We have made a sufficient examination of the properties of the aluminium cell to determine the conditions that regulate its use for the purpose suggested in this paper. Carbon-aluminium cells were used with a solution of a trace of sulphuric acid in water almost saturated with alum. Where small aluminium electrodes were used, we found that the cells would form themselves, operate continuously and cut out alternate impulses. At 33.5 cycles with impulse E.M.F S generated as specified above, and aluminium electrodes varying from .0767 to 2.41 sq. in. in series with 50 ohms, the cells rectified continuously. These phenomena of rectification were examined critically with the aid of the contact maker. In all respects the actions witnessed were precisely the same in character as those recorded by H. J. Hotchkiss[1] with his remarkable oscillograph in the process of rectifying an alternating current of ordinary form at the low periodicity of 17 cycles. The small capacity current there witnessed becomes increased with the periodicity. The impulse E. M. F.s have the character of a very high frequency alternating pressure in relation to capacity. The above impulse E. M. F.s produced at 33.5 cycles have characteristics for capacity phenomena that correspond to a periodicity of 800 cycles. It follows then that had Mr. Hotchkiss made his oscillograph records, supposing that to be possible, at 800 cycles instead of at 17 the capacity current seen in his records as superimposed upon the rectified current phenomena would have been increased 47 times. This will make clear then why we found the large capacity effects as given below.

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[1] Physical Review, March 1899, reprinted in the *Electrical World and Engineer*, July 22, 1899.

In all cases where the small aluminium electrodes as specified above were used, the rectifying phenomenon was complete and definite. In every case the superimposed capacity current referred to above was witnessed. The smaller the aluminium electrode the smaller was the capacity current and the greater was the resistance of the cell, however close the electrode was placed to the carbon rod electrode.

The following dimensions of the impulse E. M. F. and capacity phenomena pressures were observed around the terminals of a 50-ohm non-inductive resistance in series with the impulse secondary and one aluminium cell as specified. The impulse transformer was operated at 33.5 cycles and excited with 20 amperes at 40 volts delivering the impulse E. M. F. in the secondary as described above.

DIMENSION OF		Aluminium Cells.		
		A	B	C
Surface of electrode in square inches		2.41	.512	.0767
Surface of carbon electrode in square inches		3.75	3.75	3.75
Distance between electrodes in inches		.5	.5	.5
Impulse E. M. F. left standing by aluminium cell.	Maximum positive volts of impulse	16.3	14.2	9.
	Average width of same in per cent. of one-half cycle	3.5	3.5	3.
	Maximum volts negative, caused by capacity effect	2.	.8	.1
	Average width of same in per cent. of one-half cycle	5.5	1.5	1.5
Capacity E.M.F. caused by cell where impulse E.M.F. was cut out.	Maximum volts positive capacity effect	10.3	5.2	.8
	Average width of same in per cent. of one-half cycle	6.7	1.5	2.2
	Maximum volts of negative capacity effect	10 8	4.2	.4
	Average width of same in per cent. of one-half cycle	3.33	1.	1.

It appears from this table that trustworthy results can be obtained when using this cell, only by applying the smallest practicable aluminium electrode. With larger electrodes the capacity currents will introduce errors in the instantaneous measurements of wave forms especially in the neighborhood of their zero values.

Cells as constructed above but in which larger electrodes are provided, were found to be entirely useless for this purpose.

When using this method for measuring the wave forms of capacity currents involved largely in the production of the "corona" effect where the + and — alterations differ in form, we found that a commutator for cutting out every other impulse attached to Prof. G. S. Moler's cycle counter worked so satisfactorily that we preferred it to the aluminium cell. The aluminium cell with a small electrode is so easily made when one does not have at hand a cycle counter, that it will doubtless be found convenient and useful in many cases where measurements are to be made to determine the separate forms of the + and — alterations.

DISCUSSION.

MR. STEINMETZ:—I have listened with great interest to Prof. Ryan's paper giving a novel and very ingenious method of wave shape determination. I consider this as one of those elegant, and I may say artistic solutions of a problem which are of considerable interest by themselves, even outside of the problems they deal with.

Regarding the contact maker, however, there is the disadvantage that no contact maker can really give the points of the curve, but necessarily gives a certain range, since even if an instantaneous contact can be made, which is not possible, the arc following the contact for an appreciable time prolongs it. We must consider that at a frequency of 125 cycles, a contact of .0001 of a second represents over four degrees of phase and thus will blur and smooth out very high harmonics as they occasionally occur.

The only method which I found to give really instantaneous readings, that is points of the alternating wave and not averages of a short range, is the method devised some years ago by Mr. F. Holden, and in continuous use since that time in our factories. The contact maker disk consists of two segments per pole and is driven either by the alternator, or by a small synchronous motor. One of the segments establishes connection between the alternator wave and a condenser, the other section closes the condenser through a voltmeter or other instrument. As seen, in the moment where the condenser disconnects from the alternating wave, its charge is proportional to the alternating potential at this very point, but no current flows except the differential of current corresponding to the change of charge of the condenser, and no minute arc prolongs the contact. Thus the instrument reading of the discharge current of the condenser is directly proportional to the potential of the alternating wave at the very instant where the condenser disconnects from the alternator.

Dr. Sheldon:—Concerning the aluminium cell mentioned in the paper, the inference is to be drawn that there is a high electromotive force of polarization. A few experiments which I have made indicate that there is no polarization of the magnitude frequently mentioned. Instead, the opposition to a flow of current is really due to a formation of some aluminium salt on the electrode. This salt is a non-conductor, and upon reversing the current it is carried back into solution. These conclusions I have drawn from the fact that an aluminium electrode becomes quickly heated, with a very small current, showing that the considerable number of volts lost right at the surface of the plate are used in overcoming resistance with the resulting heat development rather than in producing chemical decomposition.

Mr. Steinmetz:—I had occasion lately to investigate this feature somewhat further and was struck with the almost absolute arrest of current by the aluminium cell, a potential of 15 volts sending no noticeable current through a single cell from aluminium to carbon, while from carbon to aluminium the cell acts practically as short circuit.

I have investigated the cause of this phenomenon, and believe it is due to a thin film of aluminium oxide forming at the surface of the aluminium plate, as soon as current begins to flow therefrom into the electrolyte. This film being a good insulator practically interrupts the circuit. Experience has shown me that the phenomenon occurs only with such electrolytes as do not react electro-chemically upon aluminium oxide. The efficiency of this electrolytic rectification is very small, but considerable differences exist between different electrolytes. An investigation in this direction is still in progress.

Prof. C. A. Adams:—It is my experience that too little stress is ordinarily laid upon the duration of contact in the more familiar contact-maker methods. The machines used for such work in many college laboratories are of the bipolar type, in which the mechanical arc of contact and the electrical arc of contact are the same, and it is therefore not difficult to reduce the mechanical arc to such a point that the variation of E. M. F. or current during the interval of contact, is a very small percentage of the maximum; but when this same contact maker is applied to the shaft of a multipolar alternator, the electrical arc of contact is multiplied by the number of pairs of poles, or by the number of cycles per revolution, and it becomes very difficult to reduce the time of contact to the point necessary for accuracy, if the ordinary type of contact maker is used. Some years ago I devised a contact maker which practically does away with this difficulty and which has been in successful use in our laboratory ever since. It is ordinarily used in connection with the well-known telephone method, and is illustrated in Fig. 6. Once during each revolution, the hardened steel pin, P, makes contact with the steel spring S, and thus closes the contact maker circuit through the stiff strip B, but almost simultaneously with this closing the circuit is opened by the separation of S and B. Thus the same blow that closes the circuit opens it, and almost simultaneously. The duration of the contact is so small as to be negligible, even with high periodicities, perfect silence having been obtained in a very sensitive telephone, with an alternator of 180 cycles per second.

Prof. Ryan:—I would like to add a word to the very interesting and beautiful method that Mr. Steinmetz has described,—to mention just now a use of the condenser a little different from that of tracing the forms of alternating values. Very often one wants to know just what the maximum pressure is that the insulation of a machine is strained to in practical operation. For instance in a street car motor it is of value to have a method by which we can tell what the highest electro-motive force is that the motor is capable of producing when the current is thrown off by means of the controller. If you use the condenser in series across the terminals of the motor *a b* and then through a battery of aluminium cells in this way: [See Fig. 7.]

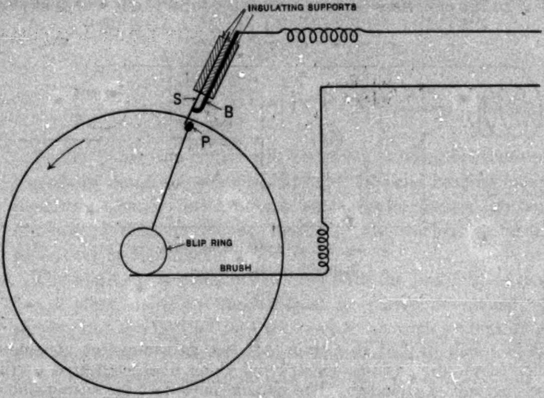

Fig. 6.

This arrangement of the cells comes to us from our German friends. Across here we put a galvanometer, *c d*; the condenser of course will be given a definite charge due to the maximum pressure the system is subjected to, and the condenser will discharge itself. The charge will come through the galvanometer in a certain direction in charging up, and it will discharge in the same direction by virtue of the action of those cells, so that you have a throw on the galvanometer that is proportional to the maximum electromotive force developed in manipulating the motor.

PROF. C. P. MATTHEWS:—Professor Ryan has referred to the admirable little galvanometer perfected by Mr. Hotchkiss. I think this little instrument and its possibilities are not sufficiently familiar to the electrical engineers of this country, and this fact leads me to speak of it at this time. It is a galvanometer of almost microscopic dimensions, constructed along lines suggested by the oscillograph of Blondel. The natural period of the suspension is so exceedingly small that the needle follows with great accuracy all the irregularities in the wave form, and one obtains on the photographic plate a trace which shows the conditions obtaining at a given instant. These advantages are secured, it is true, at a cost of considerable skill and patience in preliminary arrangements, but the results are very gratifying, and I should be glad to see the apparatus in more common use.

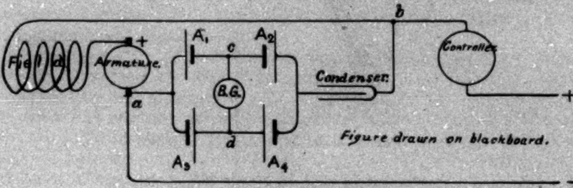

Figure drawn on blackboard.

A_1, A_2, A_3, A_4 Aluminium Cells.

B.G. Ballistic Galvanometer.

FIG. 7.

PROF. RYAN:—The remarkable apparatus of Mr. Hotchkiss corroborates the experiments made by Mr. Steinmetz with the aluminium cells.

[Adjourned to Tuesday, June 27.]

A paper presented at the Sixteenth General Meeting of the American Institute of Electrical Engineers, Boston, June 27th, 1899. President Kennelly in the Chair.

ELEMENTS OF DESIGN FAVORABLE TO SPEED REGULATION IN PLANTS DRIVEN BY WATER POWER.

BY ALLAN V. GARRATT.

In this paper the writer will endeavor to describe those peculiarities of design of plant which have a special bearing on speed regulation, but no attempt will be made to discuss the theory, mechanical construction or merits of the various water-wheel governors on the market.

The engineer is often confronted with the problem of designing a plant upon an undeveloped or partly developed water power, and the desired end is to come out with a plant of good mechanical and electrical design and yet have it such that the speed of the electrical apparatus may be maintained within comparatively close limits under any load variations which can possibly occur, and to maintain the speed within very close limits under any working load variations.

The kind of generating apparatus used, and the nature of the load, predetermines the degree of regulation which must be obtained under both accidental and working conditions, but it is quite evident that the tendency in modern plants is in the direction of apparatus which requires closer speed regulation, and more facility in handling the speed than heretofore.

It is quite possible to obtain on the market, water-wheel governors which will,—provided the design of plant is good,—give quite as good a speed regulation as could be obtained if the plant were driven by first class steam engines.

There is more than one water-driven electric plant in this country where auxiliary steam plants are used, in which the speed is fully as constant while the load is carried by the water-wheels as while it is carried by the steam plant. The plants of the Derby Gas Co., the Pawtucket Electric Co. and the Woonsocket Electric Co. may be referred to as examples illustrating the above fact.

The largest accidental load variation which can occur is evidently an instantaneous change amounting to the full capacity of the water-wheels. The working load variations may be anything less than this.

The writer has found, in a practice amounting to something over 90,000 horse power of water-wheels in the last four years, that with good water-wheels properly set and rigged, and controlled by governors of suitable design, the speed may be held within five or six per cent. of normal upon circuit breakers opening under full load, and that the speed may be brought back to normal in from five to fifteen seconds, depending upon the amount of kinetic energy in the rotative parts and moving water column. With incandescent loads of the ordinary type, a recording tachometer will show a practically straight line. With ordinary electric railway loads, speed variations of about three per cent. as a maximum may be expected. These figures are not intended to be of universal application, but are for simply showing the present state of the art. It should here be added that governors can be obtained which will permit any number of independent water-wheel units driving electrical units connected in parallel, to be operated with perfect convenience and safety. It should also be noted that in the case of alternating units it is perfectly easy to get them at speed and in step for multiple connection without undue delay, and without any hand regulation.

These desirable ends cannot, however, be obtained to their fullest extent if the general design of the hydraulic portion of the plant is bad. We will now consider those things, aside from the governor itself, which tend to make the regulation good or bad.

As a preliminary thought let us consider for a moment that the problem is quite different from steam-engine governing, which naturally comes to the mind in this connection, for the reason that water is heavy, practically non-compressible or non-expansive, and must be transmitted to the water-wheel in large volume and at low velocity; while steam is light, highly compressible and expansive, and may be transmitted to the engine in small volume and at high velocity. From this it follows that the engine valves are small, light and may be perfectly balanced, while water-wheel gates are necessarily large, heavy and are frequently,—although often unnecessarily,—out of balance. The inertia of the steam may be always neglected; the inertia of the water must be always considered.

The problem of governing a water-wheel, then, involves moving large volumes of a heavy, practically incompressible fluid acted on by the force of gravity alone, and of moving ponderous gates; and this must be done with absolute precision and great promptness. Also adequate provision must be made for the momentum and inertia of the moving water and mechanical parts.

To put our minds in a proper attitude to approach this subject, let us refresh our memories in regard to some of the relations of force, mass, velocity and time.

We have here a mass free to move. Its property of inertia prevents its moving until some force is applied to it. When, however, I apply for a moment the force of my hand it begins to move, and when I stop pushing it, it continues to move with a fixed velocity until I apply to it the same amount of force I used to put it in motion which brings it to a rest, and it cannot move again until a new force is applied to it.

I have here a pendulum beating seconds, and here, (Fig. 1) are two masses consisting of pairs of balanced weights suspended by fine wires over pulleys which have as little friction as possible. One of these masses is twice as great as the other. If we apply to them equal forces in the shape of small additional weights we will find that at the end of one second the smaller mass has acquired twice the velocity of the larger mass; or, in other words, where forces are equal the rates of acceleration are inversely proportional to the masses.

But now I have here two equal masses (Fig. 2) and if we apply to them two forces,—one twice as great as the other,—we will observe that at the end of one second, the velocity of the mass acted upon by the larger force is twice as great as that of the mass acted upon by the smaller force; or, in other words, where masses are equal, the velocities are proportional to the forces.

Now, I have here two masses, (Fig. 3) one twice as great as the other, and if we apply to the larger mass a force twice as great as that which we apply to the smaller mass, we will observe that at the end of one second, their velocities are the same; or, in other words, for equal velocities, the forces must be proportional to the masses.

Or, to generalize; velocities are inversely proportional to masses, and directly proportional to forces.

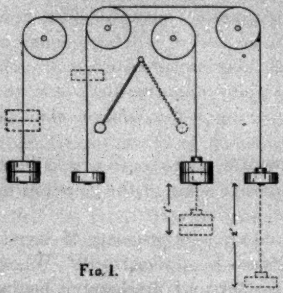
Fig. 1.

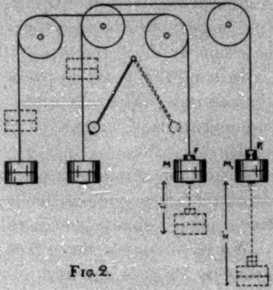

Fig. 2.

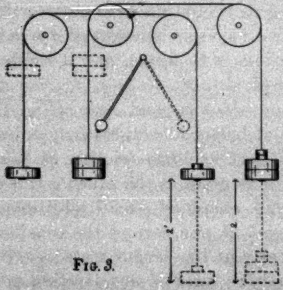
Fig. 3.

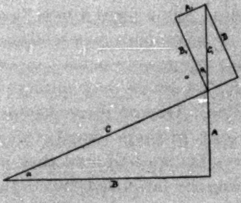

Fig. 5.

Or, by assuming the unit of force as that force which will, in unit time, give unit mass unit velocity, we may formularize the phenomena we have observed, by writing

$$\text{Force} \times \text{Time} = \text{Mass} \times \text{Velocity} \tag{1}$$

from which we may get by transposition

$$\text{Force} = \frac{\text{Mass} \times \text{Velocity}}{\text{Time}} \tag{2}$$

$$\text{Time} = \frac{\text{Mass} \times \text{Velocity}}{\text{Force}} \tag{3}$$

$$\text{Mass} = \frac{\text{Force} \times \text{Time}}{\text{Velocity}} \tag{4}$$

$$\text{Velocity} = \frac{\text{Force} \times \text{Time}}{\text{Mass}} \tag{5}$$

The product of "force" into "time" is called "impulse," and the product of "mass" into "velocity" is called "momentum"; the equation teaches us that an "impulse" is equal to the "momentum" which it produces.

It is chiefly with the practical application of the laws we have just enunciated that we have to do in regulating the speed of water-wheels.

Let us examine still further into the matter. We know that if we let any weight fall freely under the force of gravity,—or, as it is usually written, with a force $= g$,—at the end of one second it will have acquired a velocity of approximately 32.2 feet per second; or, if we throw any weight up with an initial velocity of 32.2 feet per second, the force of gravity will stop it in one second.

In the latter case, the velocity at the start $= 32.2$, and the velocity at the end of the second $= 0$, and the mean or average velocity and the distance it will travel $= 32.2 \div 2 = 16.1$ in the first second. Therefore, the work in foot pounds, which any weight can do by being thrown vertically with an initial velocity of 32.2 feet per second = weight $\times \frac{g}{2}$ feet.

Please note that in above case, the initial velocity $(V) = g$ or 32.2, and that $V \div g = 1$.

If, however, we have thrown the weight upward with an initial velocity twice as great as before: *i. e.*, 64.4 feet per second, the force of gravity would stop it in two seconds; but the mean velocity in this case is $64.4 \div 2 = 32.2$, which is twice what it was before, and we must also note that it was traveling upward twice as long as before; hence, by doubling both the velocity and the length of time, it will ascend four times as far. Thus, by doubling its $V \div g$, which in the latter case $= 2$, we have enabled the weight to do four times the work. Or, we may truthfully state that in the second case the work equals that in the first case multiplied by $(V \div g)^2$; or, formulating it

$$\text{Work in foot pounds} = \text{weight} \times \frac{g}{2} \times \left(\frac{V}{g}\right)^2 \qquad (6)$$

$$\text{Work in foot pounds} = \text{weight} \times \frac{g}{2} \times \frac{V^2}{g^2} \qquad (7)$$

$$\text{Work in foot pounds} = \text{weight} \times \frac{V^2}{2\,g} \qquad (8)$$

or we may more conveniently write it

$$\text{Work in foot pounds} = \frac{\text{Weight}}{g} \times \frac{V^2}{2} \qquad (9)$$

which is the form in which we will have occasion to most often use it.

As this is a universal law applicable to any force and any velocity it is applicable to water falling under the influence of gravity.

To fix it in our minds let us apply it numerically to the masses with which we have been experimenting. Start with the masses as shown in Fig. 2.

Let the masses M and M_1 be equal, and let them each be numerically equal to 1. Let $F = 2$ and $F_1 = 4$. Let their time of action $T = 1$ second, then their velocities at the end of the time T will be

$$V = \frac{F\,T}{M} = \frac{2 \times 1}{1} = 2$$

$$V_1 = \frac{F_1\,T}{M_1} = \frac{4 \times 1}{1} = 4$$

Now at the end of the time T, stop the action of the forces F and F_1 and apply to the masses M and M_1 in an opposite direction a new force $F_2 = 2$. This new force will stop the masses in the following lengths of time:

$$T_1 = \frac{M \times V}{F_2} = \frac{1 \times 2}{2} = 1$$

$$T_2 = \frac{M_1 \times V_1}{F_2} = \frac{1 \times 4}{2} = 2$$

But the space S_1 and S_2 through which they will travel before they stop will be

$$S_1 = \frac{V\,T_1}{2} = \frac{2 \times 1}{2} = 1$$

$$S_2 = \frac{V_2\,T_2}{2} = \frac{4 \times 2}{2} = 4$$

or the masses are as 1 : 1
and the forces applied to them are as 2 : 4
for lengths of time which are as 1 : 1
which give them velocities which are as 2 : 4
and cause them to travel through spaces
which are, during time *T*, as 1 : 2
consequently doing work on them which are as

$$\left(1 \times \frac{2 \times 2}{2}\right) : \left(1 \times \frac{4 \times 4}{2}\right) \text{ or as } \quad 2 : 8$$

They can then oppose equal forces
through spaces which are as 1 : 4
during lengths of time which are as 1 : 2
consequently doing amounts of work which are
as $F\,S$ and $F\,S_1$ or $(2 \times 1) : (2 \times 4)$ or 2 : 8

In the above case the masses M and M_1 should consist of weights of 32.2 lbs.; that is, the weight at each end of the wire should be 16.1. The force $F = 2$ lbs., $F = 4$ lbs., $F_2 = 2$ lbs. A pendulum 39.1 inches long from center of weight to point of support will beat near enough seconds for ordinary experimental purposes. A few experiments carefully carried out with the apparatus shown in Figs. 1, 2 and 3 will teach one more about the relations of mass, force, time and velocity than can readily be learned in any other way.

We are now prepared to see the application of the laws, and the formulæ we have written, directly to one of the most important details connected with the installation of water-wheels. This can be best shown by an example.

We have here two water-wheels (Fig. 4) operating under the same head, which we will assume to be nine (9) feet. You will observe that although the head is the same in both cases, one wheel,—which we will designate No. 1,—is set in an open flume of ample size; while the other wheel,—which we will designate as No. 2,—is in a closed flume connected to open water by a long closed pipe which is nearly horizontal. The behavior of these two wheels when operating under variable load, is entirely different.

Let us assume, for the purposes of argument, that the efficiency of the wheel is the same at all stages of gate, and that the amount of water which passes through the wheel is proportional to the gate opening, and that the power of the wheel is proportional to the amount of water which passes through it under constant pressure. Now, if the wheel is operating at full gate and half the load is suddenly thrown off, and the suitably designed governor attached to the wheel promply shuts the gates so that only one-half as much water can pass as when the wheel was at full gate, it is evident that the speed will remain comparatively constant.

Let us see if this will be the case with wheel No. 2. If it is operating at full load, and half the load is instantly thrown off, and the governor promptly shuts the gates so that only half as much water can pass, it is evident that the velocity of the water in the closed pipe must be reduced one-half.

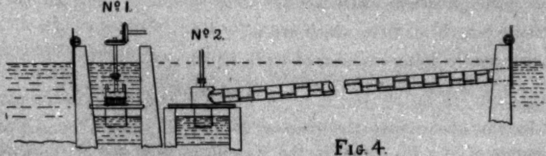

FIG. 4.

If we assume that the water in the pipe weighs 1,000,000 pounds, and has a velocity at full head of four feet per second, its energy (see formula 9) $= 1,000,000 \div 32.2 \times 4^2 \div 2 = 248,440$ foot pounds, and if the water velocity at half load is two feet per second, then its energy $= 1,000,000 \div 32.2 \times 2^2 \div 2 = 62,110$ foot pounds, and the difference between these two amounts of energy, $248,440 - 62,110 = 186,330$ foot pounds, must be expended upon the water-wheel before the water velocity is reduced to two feet per second.

If it were expended in one second it would $= 186,330 \div 550$ horse power, but this is a little quicker than we would expect to do in practice. Suppose we slow up the water column in two seconds, then the energy expended $= 186,330 \div 550 \times 2 = 169$ H. P. for two seconds. The above value of H. P. would not hold strictly true unless the rate at which the gate closed was proportional to the rate at which the water column slowed up; but the total foot pounds expended on the wheel would be as above stated. To find the exact value of H. P. at any instant of time, would require a more elaborate mathematical treatment of the problem than the time now at our disposal permits; but the significant fact to which I wish to call your attention is that this work done upon the water-wheel in slowing up the water column, is entirely independent of, and in excess of, the work which is expended upon the water-wheel when it is working normally at half gate, with the water column moving at a fixed velocity.

It is evident that the above amount of work done upon the wheel while the water column is slowing up, would tend to make the speed of the water-wheel run high if the governor only half closed the gates. In fact, the governor would have to set the gates much nearer closed than one-half; or, to speak more accurately, the governor would, at each instant of time, have to hold the gates at such a position that the power developed by the wheel, due to the working head plus the instantaneous value of power being developed by the slowing water column, equalled the load upon the wheel.

This might be found to be quite unfeasible, for the pressure developed on the closed pipe and wheel case might be dangerous, or the gate might be too ponderous or too badly rigged to permit of the requisite promptness of motion.

The maximum pressure which would be developed at any instant of time at the water-wheel, would be an impossible thing to calculate without knowing a great deal more about the venting areas and time-ratio of closing them than can ordinarily be found out in practice. All that can be predetermined is what may be called, for want of a better term, the time-average pressure. This can easily be determined as follows:—

Let $P =$ the time average pressure
$L =$ the length of the closed flume in feet.
$V =$ the water velocity in feet per second.
$T =$ the time in seconds in which the water velocity is arrested.
$K =$ the area of a square inch expressed in square feet $=$ .00694.

Then

$$P = \frac{K \times 62.4 \times L \times V}{32.2 \times T} \qquad (10)$$

It will be observed that

$$\frac{K \times 62.4}{32.2} = .01324 \text{ is a constant, call this } K_1$$

and the formula becomes

$$P = \frac{K_1 \times L \times V}{T} \qquad (11)$$

Applying this to the flume we have been discussing in which

$$L = 300$$

$$V = 2$$

$$T = 2$$

we have

$$P = \frac{.01324 \times 300 \times 2}{2} = 3.97$$

As a water column one foot high exerts a pressure of .43 lbs. per square inch, it follows that a pressure of 3.97 lbs. per square inch represents a head of $3.97 \div .43 = 9.2$ feet. In other words, if the pressure on the wheel could have been kept constant all the time the water column was slowing up from four feet per second to two feet per second, the wheel would have been working under $9 + 9.2 = 18.2$ feet of head, instead of under 9 feet of head as it should have been.

From experience we know that it is impossible to close the water-wheel gates at such a rate as to keep the pressure constant, and as a matter of fact, during some portion of the two seconds the water pressure would have been greatly in excess of 3.97 lbs. per square inch above normal, with a correspondingly large disturbance of the speed.

We may note a curious fact in this connection. With a water-wheel set like No. 2 in Fig. 4, working at nearly full gate, and if under these conditions a large portion of the load is instantly thrown off and the governor is of unsuitable design and does not compensate for the kinetic energy of the slowing water column, it may be found by experiment that the speed will run higher than though there were no governor at all. This is for the reason that for an interval of time the wheel is working under light load and a greatly increased head, and there is, consequently, a greatly increased speed; or, we may say that the amount of energy applied to the wheel under the increased pressure, even though the gate areas have been somewhat reduced by the governor, is greater than would have been the case had the gates not been moved at all.

The first remedy which suggests itself is to place large relief valves near the wheel case, so that they will open and let the water escape if the water much exceeds the static head. This would help matters somewhat upon load suddenly going off, but would it help matters upon load suddenly going on? Let us examine this matter.

Suppose the wheel is working at half load with the water column moving at a rate of two feet per second, and the whole load is instantly thrown on the wheel. The governor will promptly open the gate wide, but the water-wheel cannot develop its whole power until the water column has attained a velocity of four feet per second. To gain this extra two feet per second the water column must have expended upon it the same amount of work which it expended in losing its two feet per second, namely, 186,330 foot pounds, and this must be deducted from the work the wheel will do normally at full gate; so that the instantaneous value of power developed by the wheel while the water column is gaining velocity would equal the normal power of the wheel at full gate minus the instantaneous value of power being expended upon the water column in getting up to speed.

It is evident that the speed of the water-wheel would fall considerably below normal and there would be absolutely no remedy for it in the present state of the art. I say this advisedly and have not forgotton the question of fly-wheels, which is undoubtedly in all of your minds at the present moment.

Let us now consider how long it will take the water column to get up to speed. First let us look again at wheel No. 1 for a moment. We know that the velocity of water falling without friction may be expressed by the formula.

$$V = \sqrt{2g \times H} \tag{12}$$

where V = velocity in feet per second.
g = 32.2
H = head in feet.

taking 2 g outside of the square root sign we have

$$V = 8.025 \sqrt{H} \tag{13}$$

This is the velocity with which water should enter the water wheel.

For purposes of simplicity I have in this paper ignored the corrections which should be made for water friction on surfaces and in orifices, as they do not alter to any large extent the stubborn facts we are considering. Such corrections are beyond the scope of this paper.

Applying formula No. 13 to wheel No. 1 and assuming that the water enters the wheel without friction we have

$$\left.\begin{array}{l}\text{Velocity of water entering}\\ \text{wheel under 9 foot head}\end{array}\right\} = 8.025 \sqrt{9} = 24 \text{ ft. per second.}$$

Now, as the time required for a falling body to acquire a given velocity $= \frac{V}{g}$ we find in the case of wheel No. 1.

$$\left.\begin{array}{l}\text{Time in seconds for water to}\\ \text{acquire spouting velocity into}\\ \text{wheel under 9 foot head.}\end{array}\right\} = \frac{V}{g} = \frac{24}{32.2} = .7 \text{ second.}$$

Thus, if the gates being closed were instantly opened, the water would be doing its full amount of work on the wheel in seven-tenths of a second.—To make the above absolutely true it would be necessary to assume that the water would enter the wheel with equal freedom at all stages of gate, which is not the case, but it is sufficiently near the truth for our present argument. We are also ignoring what is known as the velocity of approach for reasons previously stated.

To make sure that our figures are right let us calculate the value of V from our fundamental equation (No. 5.)

$$V = \frac{F\,T}{M}$$

Assume a vertical water column of one sq. ft. area and 9 ft. high. Its weight is $62.4 \times 9 = 560.7$: this $= F$.

Then

$$V = \frac{561.6 \times .7}{\frac{561.6}{32.2}} = 22.6$$

which is a close approximation to the value of $V = 24$ previously found, the slight discrepancy being due to the fact that 62.4 is not the exact weight of a cubic foot of water, and 32.2 is not the exact value of g. It might also be added that the square root of $2\,g = 8.025$ which is usually given in books on hydraulics, and which was previously given in formula No. 13 is a trifle too large, and a closer approximation to truth will be obtained by calling it 8.02. By using better values we can bring out $V = 23.8$

Now, in case of wheel No. 2 the water will behave in an entirely different manner. We know that the spouting velocity at wheel No. 2 is the same as at wheel No. 1 minus the friction of the pipe. Unfortunately, we are concerned not only with the spouting velocity at wheel No. 2, but with the length of time it will take to attain spouting velocity at wheel No. 2. The water, instead of falling vertically as in wheel No. 1, runs down an almost horizontal inclined plane. A large part of the force of gravity is applied perpendicularly to the inclined plane and the small remainder is applied to shove the water down the inclined plane. A diagram will make this clear.

Let A in Fig. 5 = the head in feet from open water above the entrance to the flume to tail water level.

Let B = the horizontal distance in feet from entrance to flume to draft tube at tail water level.

Then C = the hydraulic slope.

Project $C_1 = A$ = the hydrostatic head.

Draw D perpendicular to C.

Complete the parallelogram.

Then the force C_1 (which we must remember is the hydrostatic head) is equal to the forces A_1 and D of which the latter is wholly sustained by the reaction of the plane C_1 while A is wholly effective in accelerating the motion of water down the slope C. We evidently wish to know the value of A_1 in terms of the triangle $A\ B\ C$ which is similar to the triangle $A_1\ B_1\ C_1$.

We know that $A_1 = C_1$ sine a_1. Then as $C_1 = A$ and $a_1 = a$ it follows that $A_1 = A$ sine a. Let us designate the value of A_1 so found as f.

We have seen (Fig. 3) that the time to give equal masses equal velocities is inversely proportional to the forces.

Calling T the time to acquire spouting velocity down A and T_1 the time to acquire spouting velocity down C we may write

$$T : A = f : T_1 \qquad (14)$$

from which we get

$$T_1 = \frac{T\,A}{f} \tag{15}$$

Let us apply this equation 15 to the case of wheel No. 2 in Fig. 4.

The head

$$A = 9 \text{ feet.}$$

Assume

$$B = 298.7 \text{ feet.}$$

Then

$$C = \sqrt{9^2 \times 298.7^2} = 300 \text{ feet.}$$

$$\text{Sine } a = \frac{A}{C} = \frac{9}{300} = .03$$

$$A_1 = A \text{ sine } a = 9 \times .03 = .27 = f$$

$$A = 9$$

$$T = .7 \text{ previously found}$$

then

$$T_1 = \frac{T\,A}{f} = \frac{.7 \times 9}{.27} = 23.3$$

seconds required for water to acquire spouting velocity down slope C.

It may be noted that

$$T_1 = \frac{T\,A}{f}$$

may be written

$$T_1 = \frac{T\,A}{A \text{ sine } a} \tag{16}$$

which by cancellation becomes

$$T_1 = \frac{T}{\text{sine } a} \tag{17}$$

which is its simplest form, and in which it should be used.

To make the above reasoning plainer, I have plotted (See Fig. 6) lines showing the time necessary for water to acquire spouting velocities into the two water-wheels shown in Fig. 4. Line 0-1 shows the time for water to acquire any velocity up to spouting velocity into wheel No. 1; line 0-2 shows the time for water to acquire any velocity up to spouting velocity into wheel No. 2.

It naturally occurs to one in this connection, that the water never has occasion to acquire spouting velocity in the flume of wheel No. 2: in fact, we assumed that the maximum water velocity in this flume was only four feet per second, which is only one-sixth of spouting velocity. It can be shown mathematically, and experiment proves that this does not interfere with the line of reasoning we have been following. If, instead of the end of the flume being wide open, it were five-sixths closed, the remaining sixth being an orifice (the venting areas of the water-wheel) capable of being varied at will, it would simply mean that in the flume the value of $g = 32.2$ would be considerably reduced. This new value we should calculate and call it G. We could then substitute it for the value of g we have been

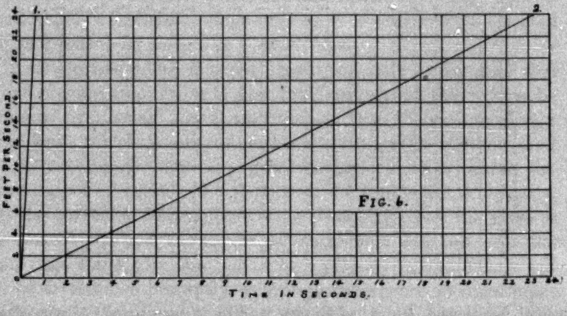

FIG. 6.

using in our calculations, and the ratio of velocity and time in the open flume of wheel No. 1 and in the closed flume of wheel No. 2 would be found to be the same that we have already ascertained.

But it may be argued that we are concerned with the force which the water will apply to wheels No. 1 and No. 2 while the water is getting up to spouting velocity, and not with the velocity of the water itself. Let us see at what rate the water will develop its full amount of energy on the two wheels we have been considering.

The theoretical amount of energy which flowing water can apply to an obstacle, advantageously placed in its path, may be expressed as follows:—

$$P = F + F = 2\ W \frac{V}{g} = 4\ w\ a\ \frac{V}{2\ g} \qquad (18)$$

where P = the theoretical energy developed
F = the force of impulse and also of reaction
W = weight of water flowing per second
w = weight of one cubic foot of water
a = cross-section of the stream in square feet.

It will be noted that if we assume $a = 1$, we may regard

$$\frac{4\ w\ a}{2\ g} = 3.9$$

as a constant, which, multiplied by the square of the velocity of flow in feet per second, will give the theoretical force which the water will develop. I have calculated the force developed at

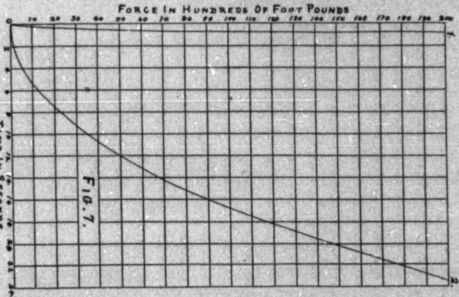

FIG. 7.

wheel No. 1 and No. 2, as shown in Fig. 4, for each tenth of a second, beginning with the water at a rest in both cases and ending with spouting velocity, and have plotted the values in Fig. 7. Curve 0–1 shows the rate at which water standing at a rest, develops its full energy on wheel No. 1: curve 0–2 shows the rate at which water standing at a rest develops its full energy on wheel No. 2. You will note in the case of wheel No. 1, how very promptly the energy gets to its maximum value, and in the case of wheel No. 2, how the energy lags for a considerable time before it arrives at anything like its maximum value.

I wish to emphasize this line of reasoning, because it is, perhaps, the most important thing to be considered in setting water-wheels where speed regulation is a desideratum. We can, in an imperfect way, provide for the expenditure of the energy necessary to slow up a water column, but there is no way to make a water column, while gaining velocity, do the work it is capable of when it has arrived at full velocity.

The important fact to which I want to especially call your attention is that the difficulty is measured not only by the length of the closed flume, but is inversely proportional to the sine of the angle of hydraulic slope. When the sine becomes 1; that is, when the angle is 90°,—or in other words, when the closed flume is vertical,—then the difficulties due to the fact that water moves slowly under the influence of gravity, have reached their minimum and the speed regulation will be the best obtainable. As the sine of the angle of hydraulic slope grows less, then the obtainable regulation grows worse.

There is one way in which the difficulties attendant upon a small angle of hydraulic slope may be in a measure compensated for, and that is by means of a stand pipe.

In an electric plant, it is not usually of such importance that a load change amounting to the full capacity of the wheels be followed by a small speed variation, as that the comparatively large loads which go off and on for short intervals of time shall not disturb the speed to any great extent. Here is where the stand pipe is of value. If a portion of the load goes off instantly, and the correctly designed governor promptly closes the gates to the correct position, the excess of water will flow out over the top of the stand pipe and the water velocity in the flume will not be arrested so promptly as though there were no stand pipe; neither will the pressure at the wheel be much increased. To obtain these results, the stand pipe should be only a very little higher than the water level in the pond. It should be located as near the wheels as possible, and its top should be turned over so that the escaping water can be led to some convenient point of discharge.

If, after a load has gone off instantly, it comes on again in a short interval of time, it finds the water velocity in the flume but little diminished, and also the vertical water column in the stand pipe is ready to apply its energy to the water-wheel in the most advantageous manner. To make the last factor of much practical use, the cross-section of the stand pipe must be sufficiently large to prevent the level of the enclosed water column from falling much while the water in the closed flume is gaining its lost velocity. As a general statement, the larger the diameter of the stand pipe and the less its height above the hydrostatic level, the better will be the speed regulation. There has not, as yet, been sufficient practical experience with stand pipes to formulate rules which will solve the least diameter which will result in any desired degree of speed regulation.

In the writer's experience, it has been found that the use of a stand pipe of ample proportions will render a plant governable within very close limits under ordinary operative conditions which had proved to be utterly ungovernable before the stand pipe was installed.

From what has been said above, it will be seen that a stand pipe is chiefly of use in aiding a good governor to maintain a comparatively constant speed under those frequently recurring load changes which obtain in electric plants,—especially in electric railway and power plants. It also gives perfect protection against dangerous water pressures being developed when circuit breakers open, or when, for accidental reasons, it is necessary to shut down the water-wheels instantly.

A stand pipe will not,—unless of very large diameter,—enable a good governor to maintain a good degree of speed regulation if the load be increased from friction load to full load instantly, where the angle of hydraulic slope is small, unless such increase of load takes place before the water in the enclosed flume has lost much of its velocity.

The writer had intended to refrain entirely from submitting designs showing the proper setting of water-wheels, for the reason that such a large number of typical plans would be required to cover all probable cases, that it would be hopeless to to treat the subject properly without extending this paper beyond proper limits. He cannot, however, resist the temptation to introduce at this point, one design (shown in Fig. 8) which has proved to be singularly adapted to the demands of water-power driven electric plants. It will be noted that the wheels are arranged for direct connection. The angle of hydraulic slope is practically 90°, giving the best possible condiitons for speed regulation. The governor may be placed directly outside the flume head and connected to the gates in the simplest possible manner. The speed variations of the main shaft may be transmitted to the governor by one belt. The requisite R. P. M. may be obtained by varying the diameter and number of wheels. The number or size of wheels on the one shaft may vary from one to as many as can be handled by one governor, or as may be required by the capacity of the electrical unit. This general design has found favor in a number of the most prominent plants in this country as well as in Europe. The regulation is invariably good if a suitable governor be used.

It is usually the case that part of the head utilized in modern plants is below the water-wheel in the shape of a draft tube: in fact, where horizontal wheels are used, it is practically necessary to have them a number of feet above tail water level for convenience of connection to the driven machinery.

The same general rule holds good in regard to draft tubes, which, we have found, applies to closed flumes. They should be as short and as nearly vertical as possible. The maximum vertical length of a draft tube is, of course, limited by the atmospheric pressure. The water stands in the draft tube for the same reason that mercury stands in a barometer. The specific gravity of mercury is 13.6: that is, it is 13.6 times as heavy as water. Atmospheric pressure holds mercury up in the barometer tube,—let us say 30 inches or 2½ feet,—therefore it will hold water up in the draft tube 2.5 × 13.6 = 34 feet: that is, it would do so if the draft tube were air tight. The external atmospheric pressure at the top of such a draft tube would be 14.7 pounds per square inch. There are few draft tubes that would stand that pressure without leaking air. This fact is well recognized by hydraulic engineers, and it is rare to find draft tubes 25 feet high from tail water level to water wheel centers. If the water-wheel is likely to be subjected to large load varia-

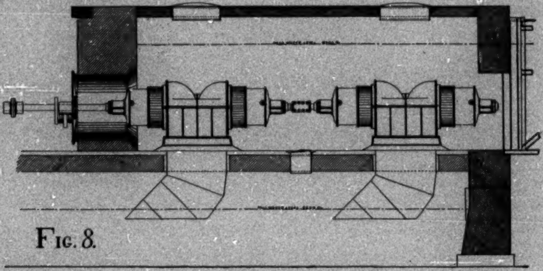

Fig. 8.

tions, it is very desirable that the draft tube should have a much less vertical height for the following reason :—

At the bottom of a 25-foot vertical draft tube the atmospheric pressure is forcing the water up with a pressure of 14.7 pounds per square inch, and the weight of the water is pressing down with a pressure of 10.75 pounds per square inch: that is, the difference between the air pressure and the weight is 14.7—10.75 =3.87 pounds per square inch. Now, if the water velocity in the draft tube is suddenly arrested by shutting the water-wheel gates the kinetic energy of the slowing water column will be found in the downward momentum of the water. This may easily create a downward pressure greater than 3.87 pounds per square inch, in which case a vacuum would be formed in the upper part of the draft tube and the column of water would sink in the draft tube and immediately after would rush upward again striking the bottom of the wheel with great violence. If we were so fortunate as to escape an accident of the kind above described we should find that with a draft tube of considerable height there is a tendency for air to leak in, and this, under the negative pressure of the weight of the water, expands into a partial vacuum so that the draft tube will be only partly filled with water, and as the position of the water-wheel gates varies as the load changes, the water column in the draft tube will sway up and down producing the effect of a pulsating head on the water-wheel. This is very detrimental to good speed regulation, and is a very common annoyance encountered in practice. The performance of such a draft tube may be easily illustrated by holding a mercurial barometer in the hand and slowly moving it up and down.

Air chambers on flumes, to give protection against water hammer effects, are of very little practical use unless of ample size, even if they are full of air. The writer examined a plant so located that the bursting of the flume would have destroyed the whole plant and ruined an investment of, at least, $100,000. At the lower end of the flume was a large air chamber. The superintendent in charge pointed with pride to it, and confidently expressed the belief that it afforded ample protection against the dangerous strains on the flume due to water hammer. Upon examining the air chamber it was found to be entirely filled with water, and it had probably been in that condition for a considerable length of time. Water under pressure absorbs air with great facility. An air chamber should be provided with an air pump which may be readily connected to some convenient source of power, and with a gauge glass to show the water level. When so arranged, and if of ample size, it affords considerable safety against pressure developed when load goes off suddenly; but it is of no practical use as an aid to the governor in maintaining constant speed.

Aside from designing the water column along the lines already suggested, so that the water may gain its working velocity in the least possible time and also so that it may add to or take from the water-wheel the least amount of the kinetic energy of the water, the next most important thing is the design of the water-wheel gates and the method of connecting them to the governor.

As has already been pointed out, the gates are of necessity large and heavy, and yet they must be moved with great promptness and precision. The writer has had occasion to investigate with more or less accuracy the number of foot pounds necessary to open and close the gates of several hundred water-wheels, and the surprisingly large variation in the amount of energy required, leads him inevitably to the conclusion that this matter has not received in many cases the careful engineering treatment which it deserves.

Water-wheels are of many designs and sizes, and work under many different conditions of head, but there would seem to be no adequate reason why the gate of one water-wheel developing a certain amount of power under a given head should require only 1,000 foot pounds to completely open it, and the gate of another water-wheel of different make, developing the same amount of power under the same head, should require 60,000 foot pounds. Yet such has been found to be the case. The above example, taken from actual practice, is by no means unusual; and scores of such cases could be cited showing relatively absurd figures.

Some builders prefer to use cylinder gates on their wheels; others prefer wicket gates; while still others adhere to register gates. It is not the intention of this paper to enter into a critical comparison of the merits of these various types of gate, and, in fact, from the standpoint of speed regulation, no such comparison is necessary for the good and sufficient reason that there are wheels on the market of all three of the above kinds which show little to be desired in the ease with which the gates may be moved. It is also true that there are makes of wheels of all three kinds which cannot be governed accurately under variable loads, simply for the reason that their gates cannot be moved quickly enough.

It is often necessary to start a gate from a rest and completely open or close it in two or three seconds, or give it a proportionately smaller motion in a proportionately shorter space of time. Or, what is still more severe, it is often necessary that while a gate is opening or closing, its motion be instantly stopped and reversed.

If one will watch a thoroughly first-class governor handling the gates of a water-wheel which is driving an electric generator operating on a variable load, one is convinced of the fact that the governor has to develop considerable amounts of energy in surprisingly short spaces of time, and that the rigging connecting the governor and the gates is subjected necessarily to considerable strain, from which it follows that the easier the gates move the less chance there is of stripping gears and twisting off shafts; to say nothing of relieving the governor itself of unnecessary strain.

All gears between governor and gate,—except immersed racks and pinions,—should be cut, of first class workmanship and not too large for the work required of them. The latter precaution is necessary to prevent the MV^2 energy in the gears themselves destroying the rigging when the direction of motion is suddenly reversed. Shafts should be of just sufficient size to give an ample factor of safety, and prevent torsional difficulties, for it is absolutely necessary that the smallest amount of motion of the governor shall be transmitted accurately to the water-wheel gate. Lost motion in gears, and twisting of shafts are fatal to good regulation. Hand-wheels should be so arranged that they may entirely thrown out of connection with the rigging while the governor is in action, or they may be unkeyed in some simple manner.

Counterbalancing a gate is not the equivalent of having it in water balance. All vertical cylinder gates are necessarily out of balance to an amount equal to their immersed weight, but that is usually so small that it is not necessary to counteract it with a counterweight.

Some designs of gate show a violent tendency to close or stay closed. It is the custom to counterbalance such gates, and this practice leads to endless trouble an account of the kinetic energy in the counterweight. It being often necessary to reverse the motion of the counterweight suddenly, the kinetic energy expended at the moment of reversal, is often sufficient to wreck the rigging. If counterweights must be used, it should be remembered that their kinetic energy is proportional to their weight, and also proportional to the square of their velocities; from which it follows that a heavy, slow-moving weight does less damage than a light, rapid-moving one.

Some general statements may be made in regard to the design of water-wheel gates adapted to plants in which it is desirable to obtain good speed regulation.

It has been the custom of late to cast onto cylinder gates, fingers reaching out between the guides. These innocent looking devices, which are supposed to guide the water into the wheel properly, and hence raise its efficiency, are a source of no end of trouble when it comes to moving the gate quickly enough to produce good speed regulation. The direction of motion of the water as it enters the wheel is always such that it presses these fingers downward with tremendous force, giving the gate a strong tendency to close. By removing these fingers, the amount of energy necessary to open the gates can always be reduced by, at least, one-half, and often times more than that. There are scores of water-wheels on record which were so much out of balance, due to the fingers on the gates, that it was found impracticable to govern them satisfactorily on acount of gears stripping and shafts twisting off. In the writer's experience, it has always been found practicable to govern these wheels by removing the fingers.

Now, as to the question of efficiency. The writer has often had to meet the argument of the few per cent. of efficiency supposed to be lost by removing these fingers, and to answer this question, tests have been made which show that there is no material gain in the efficiency of a water-wheel set under ordinary working conditions, by attaching fingers to the gate.

Two vertical cylinder-gate wheels of the same size and make, were set in open flumes side by side. The head was precisely the same in both cases. Both wheels drove electric generators of the same make, type and size. Both wheels were furnished by the maker with cylinder gates, precisely alike and provided with fingers. It was found impracticable to govern these wheels properly, on account of the gates working so hard and being so much out of balance. The fingers were removed from the gate of one wheel, and it was at once found that the wheel governed very satisfactorily under a very variable load. Then a test was made of the efficiency of the two wheels, one with, and one without fingers on the gates. Wires were brought up from the gates, carried over pulleys, kept taut by small weights, and they terminated in pointers reading on the same scale. A constant electrical load was switched onto one generator, and the position of the pointer indicating gate position was noted. Then the load was switched onto the other generator (the speed being kept the same in both cases) and it was noted that the pointer of the second unit stood at the same point at which the pointer of the first unit had previously stood. This experiment was repeatedly tried at a number of different loads, from slightly above friction load, to nearly the full capacity of the wheels. So far as could be observed, the efficiency of the wheel without fingers on the gate, was as good as that of the wheel with the fingers. The particular test above described, was made by Mr. J. H. Wilson, in the plant of the Berlin Mills Co., Berlin Mills, N. H.

The writer is aware that this test is not the equivalent of a Holyoke test, but it is certainly of great interest to the practical engineer who is harassed by the thought that in avoiding the Scylla of bad effiiciency, he will surely be wrecked on the Charybdis of an ungovernable gate.

There are a number of other details of cylinder-gate construction which time and space will not permit us to touch upon here, but which should be considered by the thoughtful engineer before making a selection. The thing to be borne in mind is that the cylinder and its connections should be of such design that they may be easily moved, and will not bind and run hard in any portion of their travel.

Wicket gates also have their peculiarities. Some makers hang them in such a manner that thay are practically in water balance in any position, and may be readily opened and closed with a small expenditure of energy. Such gates leave little to be desired, and wheels fitted with gates so designed may be governed with the greatest degree of exactness and without fear of injury to the rigging or governor. The writer has observed, however, that some wicket gates which move very easily have so much lost motion that in certain portions they tend to flop (no other word conveys the idea) first in one direction and then in the other, causing a pulsating speed which is very annoying and apparently inexplicable until one has investigated the cause. The danger of lost motion is greater with wicket than with cylinder gates, but with proper construction it is found in practice that lost motion may be entirely eliminated from wicket gates.

In some wicket-gate wheels the wickets are hinged at one end and attached by the other end by tangential arms to a banjo, which in turn is geared to the shaft going to the governor. Such gates are entirely out of water balance when partly closed, and the more they are closed the more they are out of balance. Wheels with gates of this description are very difficult to govern. Frequently the strength of the wickets and radial arms is not sufficient to withstand the water pressure, even if sufficient energy can be supplied to them. In recent practice a wheel of this description was found to require some 40,000 foot pounds to open it. Another wicket-gate wheel of different make but the same rated H. P. was found to require only 5,000 foot pounds to open it. As another recent instance, it was found that a pair of wicket-gate wheels of the kind described above required 19,000 foot pounds to open them: another pair of different make but the same rated H. P. required but 2,500 foot pounds to open them. The wheels compared above were working under the same head.

The way a maker proposes to rig his gate is a good indication of the amount of energy he thinks it will take to move it. If he thinks it is necessary to use worm gears or multiplying gears giving a large number of turns to the hand-wheel it is safe to conclude that in his opinion,—and he certainly ought to know,—the gate will move hard or be much out of balance. Such wheels it is safe to leave alone if accurate speed regulation under variable load is the end in view.

All practical engineering is a compromise between the desire of the engineer on the one hand to produce a perfect piece of engineering, and the unwillingness of the stockholders on the other hand to invest money which will not bring direct returns in the shape of dividends, or, to state the matter more conservatively, there is always a point in each plant beyond which investment must not go, and this point is different for each plant, being fixed by the economic conditions which surround the particular enterprise.

For the above reasons it is impossible to lay down hard and fast rules for the development of water powers. Assuming that the value of all engineering is measured by the dividends earned, what would be good engineering in one case is bad engineering in another. Yet, it is equally true that in an electric plant driven by water power the worst possible place to economize is in the channels and conduits which bring the water to and away from the water-wheels, and in the water-wheels themselves and their governors. Dollars saved here are apt to be expensive economy.

The infinite variety in which natural water powers present themselves makes this a difficult branch of engineering, and much is yet to be learned about it. Yet, it is safe to say that enough is now known about the subject to permit almost any naturally good water power to be so developed and utilized that the plant driven by it may be as readily controlled and its speed maintained as constant as though the driven load were carried by first-class steam engines. It should, however, be remembered that no matter how well the plant is designed, good speed regulation cannot be obtained unless the water-wheel governors are of correct design and well built. The value of a governor is measured by two things—the promptness and ease with which it will move the water-wheel gates to the correct position and stop them when they get there, and its ability to compensate by adjustment of the gate for the kinetic energy in the moving water column. One might add sensitiveness as a mark of a good governor, but nearly all modern governors are very sensitive, though most of them sadly lack the other two qualifications named above. The best made governors on the market will begin to move the water-wheel gates before any tachometer on the market will show a change in speed, yet they would not govern if they did not know when to stop as well as when to start.

I have been requested by a number of gentlemen to say something about fly-wheels. We may approach this subject in the following manner:—

When we wish to find out how much energy is stored in a revolving fly-wheel we begin by finding its moment of inertia, which is its weight multiplied by the square of its radius of gyration.

Let J = radius of gyration in feet.

W = weight in pounds.

I = moment of inertia.

$$I = WJ^2 \qquad (19)$$

The energy in foot pounds stored in the revolving wheel is as follows:

Let

$\mathcal{E}$ = energy stored in the wheel.

a = angular velocity in radians per second $= \frac{2\,n\,\pi}{60}$ (20)

where

n = revolutions per minute

and

$\pi = 3.14159$

Then

$$\mathcal{E} = \frac{I\,a^2}{2} \qquad (21)$$

Substituting the value of a we get

$$\mathcal{E} = \frac{I\left(\frac{2\,n\,\pi}{60}\right)^2}{2} \qquad (22)$$

Evolving which we get

$$\mathcal{E} = \frac{I\,n^2\,\pi^2}{1800} \qquad (23)$$

Which is the form in which the formula is ordinarily used. It may be simplified for

$\frac{\pi^2}{1800} = .00551$ is a constant.

Substituting this we get (24)

$$\mathcal{E} = I\,n^2 \times .00551$$

But you will note that this expression may be conveniently divided into two parts, as follows:

$$\mathcal{E} = n^2 \times (I \times .00551) \qquad (25)$$

Which is equivalent to saying that every fly-wheel possesses a certain quantity, which, multiplied by the square of its revolutions per minute, equals the foot pounds of energy stored in it. This quantity is its ($I \times .00551$) which we may symbolize by $\mathfrak{M}$ and we may write

$$\mathfrak{E} = n^2 \mathfrak{M} \tag{26}$$

Or, substituting our value of I we may write

$$\mathfrak{E} = n^2 (W \times J^2 \times .00551) \tag{27}$$

It is evident that this value of $\mathfrak{M}$ which is the energy stored in the wheel when making one revolution per minute when once found for any particular fly-wheel may be used at once to calculate its energy at any speed, simply by multiplying it by the square of its revolutions per minute.

This value of $\mathfrak{M}$ for the rim of any fly-wheel may be found as follows:

Let

w = weight in pounds of one cubic foot of the metal of which it is made.

d = outside diameter of rim in feet.

d_1 = inside diameter of rim in feet.

$\mathfrak{l}$ = face of rim in feet.

Then

$$\mathfrak{M} = \frac{w \, \mathfrak{l} \, (d^4 - d_1^4)}{59,814} \tag{28}$$

But a close enough approximation to the $\mathfrak{M}$ of a cast-iron fly-wheel of usual shape with light arms may be found as follows:

Let

W = weight of wheel in pounds

d = mean diameter of rim.

Then

$$\mathfrak{M} = \frac{W d^2}{23,000} \tag{29}$$

Now the question is how much of a fly-wheel do we require in any particular case. Let us return to wheel No. 1 in Fig. 4.

We remember that when the gate of this wheel was suddenly opened wide it was .7 second before the water was doing its full amount of work on the runner. We must also remember that the curve 0 – 1 (see Fig. 7) with which the water got up to full power was a parabola. The area outside the parabolic line was the work which the water did do in this .7 second, and the area inside this parabolic line was the work which the water failed to do in the same time. From the law of areas of parabolas it follows that the area inside this curve (which is a half parabola) is two-thirds of the area of the rectangle enclosing the curve. Or we may say more simply that while the wheel was getting up to speed it performed one-third of the work which it would have done in the same time had it been working at full gate and full speed.

Let us begin to apply this to the design of a suitable fly-wheel. Assume, for simplicity of calculation, that the water wheel is about 48″ in diameter and at full gate develops 100 H.P. and runs at 75 R.P.M. Let us also assume that it must not, upon the whole load being instantly thrown on or off, run more than 4 per cent. below or above normal. Its minimum speed must be then

$$75 - \frac{75 \times 4}{100} = 72 \text{ R.P.M.}$$

and its maximum speed must be

$$75 + \frac{75 \times 4}{100} = 78 \text{ R.P.M.}$$

We must also remember that it was found that we could not completely open the gates in less than two seconds. Therefore, supposing that the rate of opening the gate was uniform, the average gate opening during the two seconds was only one half, and if the wheel during every instant of time had been developing the full power due to the instantaneous value of gate opening it would have developed only one-half as many foot pounds as though it had been at full gate for two seconds. But we found that the power lagged .7 second behind the gate opening, during which time it developed only one-third of the power due to the gate opening; hence, for two seconds the wheel developed $\frac{1}{3}$ of $\frac{1}{2}$ the power it would have developed during the same time at full gate, and for .7 second more it developed $\frac{1}{3}$ of full power. Reducing this to foot pounds we have

$$\frac{100 \times 550 \times 2}{6} = 18333 \text{ foot pounds.}$$

$$\frac{100 \times 550 \times .7}{3} = \underline{12833} \text{ foot pounds.}$$

Adding these we get 31166 foot pounds, which is the total energy developed by the water-wheel in 2.7 seconds.

If working at full gate and maximum flume velocity for that length of time it would have developed

$$100 \times 550 \times 2.7 = 110000 \text{ foot pounds}$$

Subtracting from this 31166 " "

We get 78834 foot pounds, which is the amount of energy which must be developed by the fly-wheel before its speed is reduced to 72 R.P.M.

Let us assume that the fly-wheel makes the same number of revolutions as the water-wheel.

Its energy at 78 R.P.M. is

$$\mathfrak{M} \times 75^2 = \mathfrak{M} \times 5625$$

Its energy at 75 R.P.M. is

$$\mathfrak{M} \times 72^2 = \mathfrak{M} \times \underline{5184}$$

Subtracting one from the other we get 441

The value of $\mathfrak{M}$ is therefore

$$\mathfrak{M} = \frac{78834}{441} = 178$$

From the above value of $\mathfrak{M}$ should be deducted the $\mathfrak{M}$ of the water-wheel itself and the other rotating parts such as pulleys and armatures. For simplicity of calculation I shall not make this deduction in this case, and we will proceed to design a fly-wheel of suitable proportions, which shall have an $\mathfrak{M}$ value numerically equal to 178.

As it is to be of cast-iron we will limit its peripheral speed to 65 feet per second.

Let d = its outside diameter in feet.

p = its peripheral speed in feet per second.

R = revolutions per minute.

Then

$$d = \frac{p \times 60}{R\,\pi}$$

Applying numerical values we get

$$d = \frac{65 \times 60}{78 \times 3.14}\ 15.5 +$$

Assume d_1 or the diameter inside the rim = 14.5 feet

Transpose formula No. 28 so as to get ℓ or the face of the wheel in feet.

$$\ell = \frac{\mathfrak{M} \times 59814}{w \times (d^4 - d_1^4)} \qquad (30)$$

Apylying numerical values we get

$$\ell = \frac{178 \times 59814}{450 \times (15.5^4 - 14.5^4)} = 1.6 \text{ feet} = 1 \text{ ft. } 7 \text{ in. nearly.}$$

Our fly-wheel rim is, therefore, 15 feet 6 inches outside diameter, 6 inches thick, and 1 foot 7 inches wide on the face, and would weigh 16,992 lbs.

This strikes one as a very large fly-wheel for a 100 H. P. unit, but it must be remembered that it is intended to perform very severe duty. Moreover, no allowance has been made for the kinetic energy in the fly-wheel arms, nor in the water-wheel itself and other rotating parts. All of these corrections should be made and the proper deduction made from the fly-wheel above designed.

Another correction should also be made which would still further reduce the size of the fly-wheel.

It was assumed that the water-wheel was working at normal speed and friction load when the whole load was thrown on. Friction load is a part of whole load, and hence when we say we throw on whole load, we really mean that we are throwing on something less than 100 H. P. Also, at friction load the water in the flume had some velocity, and hence we did not have to start the water from a condition of rest, but from a condition of slow velocity.

To make all these corrections involves a considerable knowledge of hydraulics and mechanics. To treat this subject in a complete manner, would involve a good many figures, and would extend this paper far beyond proper limits.

It may be noted here that if it is found desirable to change the value of $\mathfrak{M}$ of a fly-wheel after it is designed, it is not necessary to re-design the wheel. The dimensions of fly-wheels are as the fifth roots of their $\mathfrak{M}$'s. We have found a fly-wheel whose $\mathfrak{M} = 178$. If we now wish to reduce its $\mathfrak{M}$ to 150, we write

$$\sqrt[5]{178} : \sqrt[5]{150} = 15.5 : \text{diameter}$$

From which we get

$$\text{diameter} = \frac{\sqrt[5]{150} \times 15.5}{\sqrt[5]{178}} = 15.0 \text{ feet.}$$

or formularizing it we get

$$D_1 = \frac{\sqrt[5]{\mathfrak{M}_1} \times D}{\sqrt[5]{\mathfrak{M}}}$$

Where

D = diameter of wheel having given $\mathfrak{M}$

D_1 = diameter of required wheel

$\mathfrak{M}$ = given value of $\mathfrak{M}$

$\mathfrak{M}_1$ = required value of $\mathfrak{M}$.

All of the linear dimensions should be treated in the same way. For example: if we have a design of a fly-wheel drawn to a scale of one inch to the foot, and in building the wheel we read the drawing as though it were one-half inch to the foot, then the $\mathfrak{M}$ of the fly-wheel will be $2^5 = 32$ times as large as it would have been if the wheel had been built according to the scale of one inch to the foot.

Where we have a fly-wheel of a given $\mathcal{M}$ and we know how many foot pounds it will be required to give up or absorb, as the case may be, we may find the resulting speed by the following formula:

$$r = \sqrt{\frac{\mathcal{E} - \mathcal{E}_1}{\mathcal{M}}} \qquad (32)$$

where $r =$ the final revolutions per minute.
$\mathcal{E} =$ energy in foot pounds stored in wheel at normal speed.
$\mathcal{E}_1 =$ energy in foot pounds required of the wheel.
$\mathcal{M} =$ energy in wheel when making one revolution per minute.

The above formula may be more conveniently as follows:

$$r = \sqrt{\frac{(\mathcal{M} \times R^2) - \mathcal{E}_1}{\mathcal{M}}} \qquad (33)$$

where $R =$ normal revolutions per minute. Applying to the wheel we have been discussing we have

$$r = \sqrt{\frac{(178. \times 75^2) - 78{,}834}{178.}} = 72$$

which is the minimum revolutions per minute which we first agreed upon.

We have seen that to make even an approximate design of fly-wheel we have required considerable data to work from, and have been obliged to do quite a little figuring, and yet the water-wheel in question was set in the simplest possible manner. Had it been set in a closed flume like wheel No. 2, (Fig. 4,) the problem would have been greatly complicated.

In view of these facts, it becomes quite amusing to note the alleged accuracy with which statements are often made in regard to the exact amount of fly-wheel which is required to give stated degrees of speed regulation upon 10%, 25%, 50%, etc., of the load being instantly thrown off or on, when it is perfectly evident that only part of the data is available which would enable only approximate figures to be made.

The one concluding crumb of comfort which the writer is able to offer, is found in the fact that in a very large practice, he has never found it necessary with a water-wheel set in an open flume, to install a fly-wheel in order to obtain a perfectly satisfactory speed regulation under any operating conditions of an electric plant. The various rotating parts of the plant, such as water-wheels, armatures, pulleys, etc., have sufficient moment of inertia and angular velocity to enable a first-class governor to hold the speed within very satisfactory limits under any sudden load changes which occur in the actual operation of the plant.

If the design of the hydraulic part of the plant is bad, it is wiser to try and improve it, rather than lean largely on fly-wheel effect, which is a weak prop at the best.

DISCUSSION.

MR. CHARLES F. HOPEWELL:—I would like to ask if the length of the draft tubes makes any difference.

MR. GARRATT:—As a matter of practical experience I have found the more of the head you have above the wheel and the less below it the better the regulation you can obtain, for the practical reason that when wheel builders don't make things tight, and when the pressure is from the inward outward it simply means that a little water leaks out, but when the pressure is the other way it means that the air leaks in. The more air you can keep out of your pipes the better regulation you will invariably get. There is no danger of air getting in above the wheel, but very great danger of it getting in below the wheel. So that it is well to put as much of the head above as you can.

PROF. W. S. ALDRICH:—Almost all of the cases given here relate to the reaction or pressure type of wheel. Of course, that is the most difficult to govern. The ordinary impulse water wheel is most easily controlled. The paper evidently proposes to consider only the type of wheels of the reaction or pressure kind. In the western states, wheels are being operated under very high heads which have brought out characteristic methods of speed regulation. Again, methods of regulation such as by throttling the discharge, have been adopted for the reaction or pressure wheels that are suitable only to that type and to be noted particularly in the Niagara plant. But the references in the paper relate entirely to methods of regulation affecting the flow.

The views shown on the screen were of a relay type of governor, a type not proposed for discussion in the paper, but which is coming into more general use for flow regulation of low and medium heads on pressure wheels. Regulating water-wheels by controlling the discharge is, of course, more particularly adapted to installations of the Niagara class; but there seems no reason why it should not be successfully employed in similar cases of smaller magnitude. It is a method of regulation not taken up in this country to any great extent on account of our very different system of manufacturing turbines. The American method is to carry stock turbines subject to telegraph order, particularly in the smaller sizes, and much in the same way as steam-engines and dynamos are held in stock. It is, therefore, a simpler thing to fit the turbine with a governor controlling the flow of water from the flume, than to make a special design of governor adapted to control the discharge.

With regard to the length of draft tube it is possible to employ, I believe they have been used 28 feet long. Twenty-four or 25-foot lengths seem thoroughly feasible if there are not very great fluctuating loads.

The matter of unsteadiness or hunting action of these water-wheels is a very difficult one to overcome, and I can assure you it is not entirely due to the mere question of the governor action alone. It has been pointed out, for instance, that in its operation the governor may be more or less like a pendulum in a condition of unstable equilibrium; unsteady in its operation on the gates, producing a variable supply of water not timed with the load changes that cause the whole plant to race. The load fluctuations I think are more responsible for this hunting or racing than almost anything else.

There are very few governors but will control a slow flow. In fact, it is not desirable to have a governor respond too quickly. Has any limit been found for the sensitiveness, so to speak, of a water-wheel governor in responding to a quick change of load? The question of lag makes the water problem a trying one to the engineer. We have made tests which seem to show that it is almost impossible to meet simultaneously the changes of load in street railway work by any kind of water-wheel governor. The steam-engine has been referred to as being capable of almost ideal regulation by the nature of the working fluid; yet, in this case even, it has been found desirable in some instances so to arrange the governor that the low-pressure cylinder of a compound engine at times of very light load may act as a drag on the high-pressure cylinder. Similarly, one turbine of a pair may be made to act as a brake to prevent sudden fluctuations of the speed at very light loads. It is an open question whether it is desirable to attempt to have any prime mover regulated for simultaneously following the almost instantaneous fluctuations of street railway work, unless such result in more or less permanent changes of load.

In the sketch, Fig. 8, of a plant arranged with a pair of turbines on horizontal shafts, there is a singular draft tube layout. It may be that the very short curved draft tubes were necessary owing to some structural arrangements of the design. Those draft tubes are, no doubt, made as shown, to meet the requirements of a design, which the author has, rather reluctantly it is true, pointed out, as representative of this class of installations. We should be careful to look at the matter of draft tubes, for it might appear that those shown here are not altogether ideal. The conical arrangement below the older types of vertical draft tubes has here been done away with, the marked curvature of the draft tube itself providing for an easy discharge of the water.

MR. C. W. RICE:—This is a very interesting and valuable paper. In most of the water driven plants the question of regulation is of the greatest importance, and there are very few plants in America that have satisfactory regulation. A requirement in many plants is economical use of water together with good regulation. In many places the reason for the poor regulation has been that restrictions on the use of water have been made. I am more familiar with high heads than low heads. In some, the heads have been as high as 1,700 feet, coming from lakes of small capacity so that the water had to be saved. I would like to know the greatest height that has been attempted so far, using a stand-pipe as an auxilary to the regulation.

MR. GARRATT:—In the first place the tallest stand pipe which has been installed for the purpose of aiding regulation is about 70 feet. Touching the economy of water by throttling it, I will say in the present state of the art it is not feasible to govern tangential wheels in this way because you can't keep a standard pressure. I am carrying on experiments now which I believe will partially rectify that difficulty, but I have not experimented sufficiently to say exactly what can be done. I hope before another summer meeting of the INSTITUTE I can point to some plants where it is perfectly possible to control the flow of water in a long flume without the use of any stand-pipe at all. Ordinary relief valves I will say are of no use.

In regard to the design of these draft tubes shown in Fig. 8 I will say that the old-fashioned draft tube, slightly conical in shape, the cone being an inverted or concave paraboloid, is almost ideally perfect. But it does not conform to commercial requirements. The tail water has got to be thrown right out in one direction. In this design here that I showed on the screen, the tail water was thrown out directly beneath the floor under each generator. This draft tube shown here is far from being ideal, yet it gives the water the right direction and starts it off in the direction it should go. If you have the room underneath you certainly should not use that form of draft tube.

With regard to the promptness with which water should be permitted to enter the wheel when load goes on: it is not my purpose here to criticise other papers on the speed regulation of turbines. I have tried to confine myself to actual practice and not talk about things which might be different if the Almighty had made different natural laws. In my experience I found that the quicker you can move the gate, provided you know enough to stop it at the right place the better the regulation will be. If the theory of the governor is wrong, the gate may start at the right time even though it don't stop at the right time. But as a matter of practice it is much more important to stop the gate at the right time than to start it at the right time. The limit of time in ordinary turbine practice is moving a gate from wide open to completely shut is two seconds. Though this cannot always be done I can point to plants where wheels as large as 72 inches in diameter are closed in two seconds. At Niagara Falls the regulation is almost identically the same as in another plant I can point to where the head is only five feet. It makes little difference what the height of the water over the wheel is, provided it is directly over the wheel. If properly designed, the wheel is intended to take water at a certain velocity and if it is designed for five feet it should take water at the velocity due to that head, and if two hundred it should take water at the velocity due to that head. In the case of Niagara Falls there is nearly two hundred feet head, and in another plant just south of there are wheels under five feet head. The wheels in both plants are of the same make and the regulation is practically the same. If you turn the feed pipe down flat you immediately introduce a lag in the water while gaining velocity, as I pointed out in my paper.

MR. STEINMETZ:—Thus to resume, the conclusion of the paper is, what those of us who have had anything to do with water-wheels have well noticed that water-wheels cannot be governed perfectly, but the best we can do is to limit the amount of water which has to be stopped and started, to a minimum, and to steady the speed as much as possible by a fly-wheel. This obviously does not apply to the type of high pressure turbine operating at constant flow of water by deflecting nozzle. Here, however, the difficulty appears how to dispose of the deflected stream since at very high pressure it wears rapidly even through thick cast iron plates.

More serious still than the momentary change of speed during rapid change of load is the liability of oscillation of the speed of the water-wheel, even with small changes of load, causing a hunting of the governing device, and thus a hunting of all synchronous apparatus and sometimes even rotary induction apparatus connected to the circuit, and a hunting of the alternators against each other, where several are operated in multiple.

Suppose the load changes, increasing a few per cent. The governor opens the gate wider, the pressure of the water column and thus the power decreases, and the governor opens the gate still wider until the water column has regained its pressure. But now the gates are open too far, the speed rapidly increases beyond normal and the governor begins to shut off again. This, however, increases the pressure of the water column and thus the power, and causes the governor to shut off still further, thereby ultimately causing the speed to drop and the governor to open again and so on. If now the governor is very sensitive and quick, the governor will over-reach further than the position it started from and cause an oscillation of speed of increasing amplitude, that is hunting, which usually makes the whole system inoperative. If, however, the governor is supplied with a powerful damping device it acts too slowly for rapid changes of load and thereby makes the operation of the system unsatisfactory. A governor is therefore, necessary, which acts very quickly, as quick as possible, but does not complete its regulating stroke but hesitates and gradually and very slowly settles down into the position corresponding to the changed load without overreaching it

Mr. Garratt:—For reasons which must be obvious I distinctly said at the beginning of this paper that it was not upon water-wheel governors; hence I ignored that whole subject. If the gentleman wishes I can project upon the screen photographs of the parts of a governor showing how that action is provided for mechanically with the utmost precision. I intentionally avoided that part of the subject.

Dr. Bell:—I think this subject is very vital. We have all sorrowed over it. But it is a fact as a matter of practice that in spite of the inherent difficulties of the problem, if the hydraulic conditions are fairly good, it is possible to get a good regulation of the water even in cases where the load is a railroad load and badly fluctuating at that. I have watched the operation of governors and seen railroad plants so operated as to give as good regulation as would ordinarily be got from an engine. Of course a governor must not hunt. If the governor, be it quick or slow in action, follows up too far the changes in the flow of water it will suddenly go beyond its proper regulating point and then hunt back and forth. As a matter of fact, the process which Mr. Steinmetz alluded to, the process of checking the action of the governor a a little before the work is completed, is applied systematically in at least one governor and it is the key of the situation. If a governor is free to hunt, you get bad regulation in nearly every case. The more nearly dead-beat the governor is the better the results. I don't think any relief is to be had by saying to yourself if I can't get one per cent. regulation and can get ten I will set my governor for ten. The more promptly the governor works and the better the arrangement of the hydraulic plant with respect to promptness in bringing the water pressure down, the better net result will be got on the whole. If you are working with a governor which is prone to hunt, which gives bad results—too much promptness is not desirable, but that particular governor is one it should be our object to suppress. As a matter of fact the railway plants which are driven by water are capable of giving substantially the same regulation as given by the governor of an ordinary engine; better results than with some Corliss engines. When it comes to dealing with high heads where water is wasted, the problem assumes a different form, but after all it is governed by the same considerations; you must follow out the fluctuations by the swinging of the nozzles on the buckets and there you are liable to encounter hunting unless you are very careful.

It is, however, a more difficult problem in spite of the enormous water-velocity. As regards the matter of fly-wheels, I am particularly glad to have Dr. Garratt bring a few figures before us regarding the utter uselessness of fly-wheels in many cases. There are many cases in which you can store power, but I find advantageous cases are comparatively few. A little fly-wheel capacity is all right of course, but as to putting on fifteen or twenty tons of metal and the bearings to support them, simply for the purpose of enabling you to use bad hydraulic designs and bad regulating governors, it is to my mind a waste of good material and money. And that is continually done. As I go around through the electric plants driven by water-wheels I am bound to say there is more bad design in regard to the hydraulic plant, than good design. These facts that Dr. Garratt exhibits regarding the relation of hydraulic design to the possibility of governing are not generally understood, and the feeling generally is: "Well this is a pretty good hydraulic plant and we have got what we think is a pretty good governor, if it doesn't work well, put on a fly-wheel." And the worse the design the bigger the fly-wheel.

The more directly you can get the water at the wheel the more quickly you can work your governor gates, the better generally the net result will be, and as regards the matter of quickness, I think the figures shown ought to convey to us a lesson. I should suppose off-hand that no human being in his sound senses would undertake to make an apparatus which required regulation with the maximum possible amount of over-balance. But over-balance seemed to be a virtue in the mind of the man that made that wheel. With respect to these high-head wheels I should like to throw out an inquiry into the air, merely to awaken the interest as to why we do not use in this country the impulse turbines. That type of wheel is almost unknown here. All the turbines we have with very few exceptions are pressure turbines such as have been shown on the screen. The impulse turbine is comparatively easy to govern and it does save water. Why in the world that form of wheel is not used in this country, particularly on the midheads where it gives a convenient combination of power and speed, I don't understand. On one or two occasions I have raised inquiry regarding these impulse turbines and I have regularly met the pleasing information that they are not to the obtained in this country except I think from one small concern on the Pacific coast. Abroad they are used very extensively and some firms make almost a specialty of the impulse type of turbine. I think this is a subject that ought to be looked up in the interest of an economic use of water.

Prof. Goldsborough:—There is an element of skill which enters into the economical handling of these plants which has a very marked bearing upon the success that at-tends them. Our attention has been called to one or two plants during the discussion, in which regulation is secured to the perfect satisfaction of their directors and as long as these plants are operated and controlled by men who understand them I have no doubt that they will continue to give every satisfaction. There is nothing more pleasing than to learn that in the west we have such plants as the Cottonwood and the Pioneer, located some thirty miles distant from one another, operating in synchronism in what seems to be a perfectly satisfactory manner. I understand that the engineers in charge are even sanguine of being able to operate a two-phase plant in synchronism with the two three-phase plants, by converting the two-phase currents to three-phase through the medium of transformers. In connection with the Minneapolis plant, I am not sure that its regulation always comes up to the expectations of the designers. I have understood, for instance, that at times, when the skill used in controlling the plant is possibly not the best, the results are somewhat disastrous. In other words there is a surging between generators located in the same station, and this at times is so serious that it extends through to the city sub-generating-station and the distributing system and causes more or less prolonged interruption of the street railway traffic. It seems to me, then, that even though the water-wheel governor be of very fine design, very sensitive and perfectly capable of taking care of ordinary conditions, if through bad management of the electrical machinery uncalled for conditions are imposed upon the plant, oscillations may be set up which result in placing the water-wheel governor at a decided disadvantage. You will all agree that in plants of this character, a number of which I know of in the west; in the interests of economy, there should be the requisite skill employed by the owners to the end of having them properly controlled from day to day, instead of simply employing talent to put them in shape and then removing the brains and expecting the plants to operate in a satisfactory manner. Many of us know of such instances, but fortunately they are growing fewer every day.

MR. C. W. RICE:—Professor Goldsborough speaks of an interesting experiment that will probably be made on the plants in Utah, the Utah Power Co., the Pioneer and the Big Cottonwood. Each of these plants have very long pipes leading from the reservoirs to the wheels. Therefore they correspond to the cut, Figure No. 2. The pipe at the Pioneer plant is 6 feet in diameter and considerably over a thousand feet long. It has relief valves on the pipe near the water power station. There they have the impulse type of wheel. The Utah Co. and the Big Cottonwood have the Pelton wheel, the latter company employing the deflecting nozzle and the Utah Co. employing the shaded nozzle. They have a fly-wheel in the Utah Co.'s plant, so it will be interesting to see these three stations, considerably over thirty miles apart, operating synchronously.

Mr. H. Ward Leonard: A great many plants may have the necessity of a certain amount of steam in conjunction with them. Sometimes for heating purposes, sometimes as an auxiliary to the water power, and in that connection I can say that a good many years ago, between 1883 and 1889, I installed a great many plants where water power was in use and where a certain amount of steam was used as an auxiliary. As a means of overcoming this trouble of the governor, which has been discussed, I resorted to keeping the steam engines in operation, driving upon the same shaft as the water-wheels, and the cut-off of the engine governor was kept so adjusted that the steam consumption was practically that of friction load. The gates of the water-wheel were operated by hand, and the governor of the steam engine supplied the governor for the entire equipment. in case of a sudden load the engine would instantly assume the additional work required, and in case the load was thrown off, the engine would act as a brake on the system. And in places where during the winter months heating the station is required, you practically use no more steam than would be needed for heating.

Mr. W. J. Hammer:—In connection with this subject I think it would be interesting to hear of an experiment tried in 1888 in Florida, this being I believe the first experiment made in using water flowing direct from an artesian well for power purposes. I had the pleasure in connection with Mr. John Kennish of putting in a hydraulic plant in which a turbine was placed directly over an artesian well and which drove a belt-driven dynamo. There was no governor whatsoever placed on the turbine and the power was so steady that the candle-power of the incandescent lamps scarcely varied in the slightest degree, and while this was but a small plant supplying as I recollect it, 88 16-c. p. lamps, it had some interesting features. The entire wiring system of the hotel which supplied about 8,000 16-c. p. lamps was connected to this dynamo at times, and as this hotel was not supplied with any gas or other illuminant, during the following summer when the hotel was practically closed up, and in the hands of watchmen and caretakers, lamps could be turned on anywhere in this entire system and supplied from this water power plant. The plant was also used for supplying a large garden fète held in the grounds of the Ponce de Leon hotel shortly after its erection. I understand there is a plant working quite successfully in Florida supplied with power taken directly from artesian wells and there is one in the west. But the experiment I refer to was I believe the first one ever tried in this direction and it certainly worked with remarkable success, though at the time it was installed it was stated to be an impossibility by various engineers whose opinions had been asked. Those who are familiar with this section of Florida know that any amount of water under considerable head is encountered in drilling artesian wells. Geologists claim that this water has its source a thousand miles or more away. I don't think this little plant has ever been referred to in public print, and I thought it would possibly be of interest to bring it up at this point.

[COMMUNICATED AFTER ADJOURNMENT BY S. L. G. KNOX.]

Dr. Garratt's paper is an excellent presentation of the subject of water-wheel speed regulation, but there are one or two points on which I would take exception to the author's deductions.

In formula (13) the expression $V = 8.025 \sqrt{H}$ is given as representing the velocity with which water should enter the wheel. In the next paragraph reference is made to water friction as the only factor modifying the above. Agreeing that this factor would not notably affect results of calculations, I consider the statement misleading. Only in case the turbine under consideration were an impulse wheel would the above formula be even approximately correct. As there are few impulse wheels built in this country except Pelton wheels, and as the whole paper evidently refers to the ordinary turbine plant for generating electric power—almost invariably reaction turbines—it would seem that deductions based on conditions non-existent in the type of wheels used in American plants would be misleading. In the ordinary American reaction turbine, the velocity of the water would be much less than the speed given by the above formula, and this difference, carried through the calculations, would be found to profoundly influence the results obtained.

But even assuming that the above point need not be considered, or that impulse wheels, in which the water *does* leave the guides at approximately spouting velocity, are under consideration, the subsequent reasoning seems to be incorrect. Following the author's calculations on pages 394 and 395, we would be forced to conclude that the time required for the long column of water to get up sufficient speed to supply any new demand from the wheel for more power would be the time required for the same column of water to get up spouting velocity from a state of rest if the end of the feeder were open. That is, if the water in the pipe were flowing at say two feet a second, and a sudden opening of the wheel gate called for a supply of water such that a speed of three feet per second in the main feeder would be necessary, it would take, in the given instance, 23.3 seconds for this new velocity to be attained. Now in no case, except that of a sudden change from no load to full load, would anything like 23 seconds be required for the column to get up the needed velocity. As specifications for the governing of water power plants rarely call for close regulation for a change of more than half load, the time required to accelerate the column in case of a change of full load need scarcely be considered, and in any event should not be considered as representing time required for *any* change.

Let us take a case in which it is desired to go from no load to half load instantly, and determine the time required before the water can supply the required energy. For purposes of comparison, let us take the same data as that given in the paper, for the wheel with 300 feet of feeder. In the first place, no question of regulation enters in unless the wheel is already running—that is, the wheel must be supposed to be revolving at normal speed, but only under friction load, when the sudden demand for power comes upon it. It is not likely that the friction load of wheels, shafts and generator, besides stuffing box friction, will amount to less than 8 per cent. of the full load. We must also remember that when the wheel is being called upon for only 8 per cent. of its power it is invariably working at a very low efficiency—certainly not over 25%. That would mean that the water passing through the wheel is four times 8%, or 32%, of that theoretically required to run wheel at full load. As the full load efficiency will be say 80%, the water required at full load will be 125% of the theoretical. At 8% load, therefore, the wheel will be taking 32/125=25½% of the water required at full load. Assuming half load efficiency to be 65%—the water required would be 61% of full load water consumption.

We therefore have the following conditions. Water is flowing through the wheel to the amount of 25% of that required to give full power, and a sudden demand is made for 61% of the same amount. Let us assume, as does the author of the paper, that the water enters the wheel at spouting velocity. The gate now opens, and, the feeder being unable to instantly supply more water, the 25% of normal flows through an opening larger in the proportion of $\frac{61}{25}$ than that through which it flowed when it had spouting velocity. It hence immediately takes a velocity $=\frac{25}{61}$ of spouting velocity, for it certainly will fill all the opening presented by the gate. We therefore, in the case of half load going on, have instantly the condition of water flowing through the wheel at $\frac{25}{61}$ of spouting velocity, and hence only the difference, or $\frac{36}{61}$ of spouting velocity has to be given to the water by the action of gravity on the water in the flume. Taking 23.3 seconds as the time necessary to give full spouting velocity $\frac{23.3 \times 36}{61} = 14$ seconds, would be the time needed. This is very much less than the 23.3 seconds that would be needed if the methods of calculation given by Dr. Garratt were followed.

The preceding argument assumes that the gate is quickly opened to the exact place required for half power, and left there. As a matter of fact, however, a modern water-wheel governor, under the above conditions, would open the gate as far as it would go, and later close it. We would then have a somewhat different condition. The water flowing when load came on would drop to $25\frac{1}{2}\%$ of spouting velocity, for gate would open to $100/25\frac{1}{2}$ times its original amount.

As the normal speed of the turbine would be only about 50% of the speed at which it would run with gate wide open and no load on the wheel, it is evident that the large volume of water which would flow with gate wide open would not have to flow at spouting velocity in order to give the same power to the wheel which 61% of the same cross-sectional area of water would give when flowing at spouting velocity. That is to say, with gate wide open, wheel would give half normal power at normal speed under considerably less than normal head, the wheel of course in this case running at more than 50% of the free speed corresponding to the lower head and hence at less than best efficiency. We can assume that under conditions outlined above, water would not have to attain a velocity more than 70% of normal spouting, to enable wheel to furnish 50% of normal power.

Before being able to supply the one-half of normal power required, therefore, the water in the flume would, under the action of gravity, have to accelerate from $\frac{25\frac{1}{2}}{100}$ to $\frac{70}{100} = \frac{44\frac{1}{2}}{100}$ of spouting velocity, requiring $\frac{44\frac{1}{2}}{100}$ of $23.3 = 10.4$ seconds, or less than one-half time deduced by methods given in the paper.

It can easily be shown that the change from no load to half load is a much more severe condition than from half load to full load. In the latter case we have $\frac{61}{100}$ water flowing, and suddenly open gate to $\frac{100}{100}$, thus reducing velocity to $\frac{61}{100}$ of spouting. We thus have only to accelerate water $\frac{39}{100}$ of spouting velocity requiring $\frac{23.3 \times 39}{100} = 9.1$ seconds. As, however, most modern turbine installations have some margin of power in the wheels, the governor will in the case of full power demand, open the gate wider than is required to sustain the power, and the time of acceleration will be correspondingly shortened, as was shown in the case of half-power.

Had the usual case of reaction wheels been considered, where the water never attains, normally, anything like spouting velocity in entering the wheels, the time in seconds necessary for the feeder water to accelerate would have been shortened still more—say to two-thirds of the values deduced—6 seconds, as against 23 in the paper. These values, we also believe, will much more nearly coincide with experience than those deduced in the paper. Plants which by the methods of the paper would figure out even more than the one given, are by no means uncommon, yet the insurmountable difficulties of governing, such as the necessity of finding some source of stored power other than the water to keep wheels up to speed for 23 seconds are not met with, for the wheels *do* govern under changes of 50% load, and that with fly-wheels not carrying sufficient energy to tide over more than a very few seconds without losing more than the allowable speed.

The author of the paper also refers to the use of stand pipes, and to the lack of sufficient practical experience to determine the least diameter which will result in any desired degree of speed regulation.

I am of opinion that this matter of stand pipes is subject to calculation, and, having recently been over the ground in an exceedingly interesting case would be glad to discuss the matter with some detail. The subject is too large to attempt here, and will be taken up later.

Dayton, O., Sept. 11, 1899.

THE CHAIRMAN: Is there any further discussion upon the paper? If not, we will proceed to the next paper upon the programme which is "The Protection of Secondary Circuits from Fire Risks," by Dr. Hutchinson.

Dr. Hutchinson read the following paper:

A paper presented at the Sixteenth General Meeting of the American Institute of Electrical Engineers, Boston, June 27th, 1899, President Kennelly in the Chair.

THE PROTECTION OF SECONDARY CIRCUITS FROM FIRE RISKS.

BY CARY T. HUTCHINSON.

The best protection of secondary circuits from the dangerous consequences of an abnormal potential in these circuits, has long been the subject of discussion. In the early days when the house-to-house transformer system was general, many fires were caused by the failure of the insulation between the primary and secondary coils of transformers; this was then the chief cause of such accidents. No secondary distributing networks were then used, and consequently fires following a failure of insulation were confined to the circuits from the single transformer; it was unusual for a wide-spread fire to occur, and public attention was not as strongly directed to the matter as it has been of late.

In the last few years the use of alternating current systems has increased greatly; individual transformer systems have been replaced in many places by the use of "banked" or "grouped" transformers, with secondaries feeding a low-pressure distributing network. Such systems are now in operation in many places in the country, and their number is increasing. The secondary distributing networks may be of two, three, or more wires, the latter for two or three-phase systems. The secondary circuits in all such systems are directly liable to the incursion of the high primary pressure through the failure of the insulation at some point.

In addition to alternating current systems, there is another class of distributing systems subject to the same dangers—systems in which transformation is made from high to low pressure alternating current, and thence by means of synchronous converters to low-pressure direct current for distribution. The use of such systems is growing rapidly; many of the Edison illuminating companies are already employing it, and it is a safe prediction that all the large companies will be obliged to adopt methods of this character in the near future.

Such a system has precisely the same conditions of insulation between the low-pressure direct current distributing mains, and the high-pressure alternating current feeders, as a plain alternating current system, since the low-pressure side of the alternating current supply is connected metallically to the armature of the synchronous converter which feeds the low-pressure direct current distributing system. In a word, there is only one insulation between high-pressure alternating current feeders and the low-pressure direct current network; hence such a system is liable to precisely the same risks as is the straight alternating current system.

There are a number of ways in which the high-pressure alternating potential can get to the secondary system. The most obvious is the failure of the insulation between the primary and secondary coils of the transformer; another is the contact of the high-pressure feeder and the low-pressure distributing main which may run on the same pole line, or in the same conduits underground. Wires that in ordinary circumstances seem to be secured beyond the possibility of contact may be brought into contact through unusual conditions, as in a heavy storm, or by foreign wires falling across them.

The effect of the failure of insulation, however caused, depends upon the condition of the secondary system. A brief statement of the various possible consequences may help to make the matter clear.

Any circuit, overhead or underground, carrying alternating currents, is a condenser, of which the capacity depends upon the point of view. In an overhead circuit, there is a certain capacity between the two wires, considered perfectly insulated, each as the plate of a condenser; there is also the capacity of each of these wires referred to earth, each wire being regarded as one plate of a condenser, and the earth as the other plate. The capacity in the first case fixes the difference of potential between the two wires for a given charge, or conversely for a given difference of potential between the wires fixes the charge.

The capacity in the second case—that is, regarding the earth as one plate of the condenser, fixes the potential of the wire above or below that of the earth for a given charge, or conversely. The capacity in these two cases need not have the same numerical value.

The same general considerations hold in the case of underground conductors; there is the capacity between wires determining the difference of potential between the wires, and the capacity of each wire and earth, determining the difference of potential between that wire and earth. The difference of potential between wires of a circuit transmitting energy is fixed, and the capacity is determined by the dimensions of the circuit hence the charging current is fixed. This charging current is the current on the system when no energy is transmitted and there is no leakage.

Calling V_1 and V_2 the potentials above and below the earth respectively of the two wires, and C_1 and C_2 their capacities, the equation

$$\frac{V_1}{V_2} = \frac{C_2}{C_1}$$

will hold in all cases. If one wire is connected to ground, that is, if capacity C_1 becomes infinite, the potential V_1 becomes zero. Under ordinary circumstances, the capacities of the two wires are substantially the same, and the potentials, so far as determined by the capacities, are equal and of opposite sign with respect to the earth.

Leakage over the insulators to earth has a similar effect, tending to fix a relation between the potentials of the two wires and the earth; that is, it tends to maintain the potential of the two wires at a certain value above or below the earth, but in no way affecting the difference of potential between the two wires.

Calling the conductance of the two leakage paths to ground G_1 and G_2 the equation

$$\frac{V_1}{V_2} = \frac{G_2}{G_1}$$

holds, and if G_1 is made infinite, or one side is grounded, the potential of that side becomes equal to the potential of earth. If the conductance of the leakage paths on the two sides of the circuit is the same, the potentials tend to become equal and opposite with respect to the earth.

The resultant effect of leakage currents and of the static capacity of the overhead system tends to cause the potential of the two wires to become more or less approximately equal and of opposite sign with respect to the earth when the circuits are symmetrical and insulated. In a circuit having a difference of potential of 2000 volts, the difference of potential between either wire and the earth is 1000.

The primary circuit then forms a condenser; any insulated conducting body in contact with either of the primary conductors, becomes a part of that conductor, and therefore part of one plate of the condenser, and at once acquires the potential of the wire with which it is in contact; that is, if an insulated secondary system is by any means brought in contact with an insulated primary system in which the potential difference is 2000 volts, this secondary system at once acquires a potential of 1000 volts above the earth. If the secondary system in addition has an electromotive force acting in its own circuit, then the difference of potential between this secondary circuit and the ground is increased or diminished by the voltage of its own circuit; in the extreme case, a difference of potential of 1000 volts plus the voltage of the secondary circuit, (say 100 volts) would exist, or, in all, 1100 volts between one side of the secondary circuit and ground; between the other side and ground the difference of potential would be 1000 volts. It is important to note that this difference of potential exists when the primary circuit is insulated. While this is true, it is also true that no current other than the charging and leakage current of the system can pass through the secondary system unless the primary system is grounded on the side opposite to that making contact with the secondary system. The charging current of the condenser will be practically inappreciable in all cases. For instance, at a frequency of 60 cycles per second, with a primary pressure of 2000 volts, the charging current is only three-fourths of an ampere for each microfarad capacity of the primary system. As the capacity of the primary system will probably be a fraction of a microfarad, it is clear that the passage of this current through the secondary system cannot possibly be the direct cause of danger.

The danger then lies, not in the application of an abnormal pressure to a well insulated secondary system, but in such application to a secondary system where the insulation is too weak to stand the stress brought upon it. In ordinary house wiring systems designed for, say, 110 volts, the application of 1000 volts may or may not break down the insulation from ground, depending upon the care with which the work was done and the goodness of the materials used. If the insulation of the secondary system from ground breaks at only one point and if the break is of such a character as to dead ground the secondary, then the abnormal pressure is at once relieved; the maximum potential above the earth that can exist on the secondary system as soon as this ground is formed, is the voltage of the secondary system; the moment the ground is formed, the danger is removed,—abnormal current cannot flow and consequently no fire can begin.

Assume, however, that the insulation of the secondary system is broken down at two or more points on opposite sides. In this case, a short-circuit is formed of resistance varying between zero and some higher value. The electromotive force acting in the secondary circuit, either alternating or direct, sends current around this short-circuit, in amount depending upon the resistance of the short-circuit. The current continues to flow until the grounds are entirely removed, or until the circuit is isolated, by fire or otherwise, from the source of supply. This abnormal current flowing around the short-circuit causes the fires; at every point where the current flows to ground, more or less energy is liberated in the form of heat, and fires will occur if local circumstances are favorable. The application of an abnormal pressure to the secondary system, in all probability will break down the insulation of the secondary system at several places, and these places will be distributed between two sides of the secondary system, as a matter of probability.

If the primary circuit is grounded on one side, and contact is made between the grounded side and the secondary, no harmful consequences can follow. If contact, however, is made between the insulated side and the secondary system, the full difference of potential of the primary will exist between the secondary and the ground,—that is, twice the difference of potential that existed in the case of the insulated primary. To all intents and purposes, the difference in effects caused by grounding one side of the primary circuit, will not be material. One-half of the difference of potential of the primary circuit will probably be as effective as the whole.

Although alternating current electric lighting systems have been in use in this country for a number of years, no definite practice has been followed for the protection of secondary circuits. The chief improvements that have been made in the art, have been in the character of the insulation between the primary and secondary coils of the transformer. After careful investigation as to the present practice here, I am in a position to say that in practically no case is any device regularly used to protect secondary circuits against abnormal pressure. A number of station managers have attempted to use, and in a few cases have used, devices such as I am about to describe, for a short time. The outcome of all such work has been, in general, unsatisfactory. The need of some effective method for protecting secondary circuits is well recognized, yet there is at present probably no device for the purpose that is thoroughly trustworthy and efficient in all circumstances.

The means that have been employed for this object, may be classified generally under three heads,—

1st.–Devices intended to ground, short-circuit or open-circuit the secondary circuit when subjected to an abnormal difference of potential.

2nd.–Grounded metallic shields interposed between the primary and secondary coils of the transformer.

3rd.–Permanent grounding of the secondary system.

Under the first class are the Cardew earthing device, in which a flexible metal strip is attracted to a fixed ground plate by the action of the abnormal potential, thus grounding one or both sides of the system; the Thomson film cut-out, in which an insulating film designed to withstand the working pressure, but to puncture at an abnormal pressure, is connected on one or both sides of the circuit between two plates, one connected to the conductor, the other to earth; the Stanley automatic circuit breaker, in which a flexible metal plate is attracted by the abnormal pressure to a fixed plate, thereby closing a local circuit through a solenoid, which acts to open the main switch on the secondary circuit; and others of similar character.

These devices all have one fundamental weakness,—they do not prevent the existence of the abnormal potential, but merely aim to remove it quickly; they all require time to act,—the secondary system acquires the abnormal potential at all points at the same instant, and there is no certainty that the insulation of the circuit, say at a combination fixture, may not give way before the device acts. Further, those that act to ground and to short-circuit the secondary, depend upon the blowing of fuses before the cause of danger is removed; those that disconnect are subject to all the criticisms brought against high-voltage quick-acting switches,—in particular, the maintenance of the arc.

I believe that all devices under this class are, at best, temporary makeshifts.

The grounded shield is obnoxious to various criticisms,—it is not effectual with a cross outside of the transformer, and therefore is of no value in many cases; it must be heavy enough to dissipate the energy of the short-circuited primary quickly; otherwise it melts and is itself the cause of a fire; it degrades the transformer, tending to cause unsatisfactory service, particularly in the regulation.

The third method, grounding the secondary permanently, is the only sure way to prevent the potential above earth of the secondary system rising above the voltage of the circuit. With a system grounded on one side, the maximum difference of potential that can exist between any point of this system and earth under any circumstances, is the voltage of that circuit. No cross nor any failure of insulation of the primary or secondary circuits, can raise the difference of potential between the secondary circuit and ground above this. That side of a primary circuit touching a grounded secondary circuit, at once acquires the potential of the ground or a potential differing from that of the ground by the voltage of the secondary circuit. The grounding of the secondary circuit thus absolutely ensures the safety of the circuit as regards abnormal pressures; it makes permanent the condition that the various protective devices seek to establish,—devices that may or may not work, depending on many conditions.

It would seem that a remedy as simple as this,—one generally applicable,—would have been applied in all cases; but the fact is that it has attained a limited use only. The reason for this state of affairs is the refusal of the Board of Fire Underwriters to authorize the practice of grounding any part of a circuit carrying current. The underwriters take the position that to ground one side of a circuit brings about an increased liability to fire, because the full voltage of the circuit continually acts upon the insulation of the circuit, instead of one-half of this voltage, as in an insulated system, and because one accidental ground on the insulated side may cause a fire. The danger with grounded circuit is said to be increased by the fact that the full voltage of the circuit instead of one-half, acts upon the insulation of the insulated side. This argument implies two things: first, that insulation good enough to withstand half the voltage of the circuit is not good enough to withstand the full voltage of the circuit; or, that danger would be materially increased by having the full voltage of the circuit on the insulation, instead of one-half. This is contradicted by the fact that the underwriters' rules class all circuits having a difference of potential of 300 volts or less as low potential, to which the same rule applies. That is, in the case of an insulated 110-volt circuit, which means 55 volts on the insulation of each side, the same rules hold as in the case of 220 volt circuits, with 110 volts on the insulation of each side.

If the 110-volt circuit had one side grounded, there would then be 110 volts on the insulation of the ungrounded side. It is true that the classification ostensibly permits only 150 volts on the insulation of one side of the circuit; hence, if one side of a 220-volt circuit is grounded, the stress brought upon the insulation is greater than is seemingly permitted; but as a matter of fact, the factor of safety in work of this kind is much greater than called for by this slight difference. There is no doubt that a wiring system properly installed in accordance with the rules laid down by the underwriters, is perfectly safe with 300 volts on the insulation on one side, and practically speaking, it is just as safe with 300 as with 150 volts; if the former would cause trouble so would the latter. I have assumed that one conductor of a two-wire system is grounded. If the middle point of the secondary coil of transformer be grounded instead of one wire, then the stress on the insulation is in all cases one-half of the voltage of the circuit, precisely as in the case of the grounding of the neutral of a three-wire system, mentioned below.

The main objection brought against grounding is, however, not the increased stress brought to bear upon the insulation, but the fact that only one additional ground is required to establish conditions favoring a fire. The argument is, that the simultaneous existence of two grounds on the opposite sides of the circuit is less probable than the occurrence of a single ground on the insulated side of a grounded circuit.

I do not consider these arguments strong for several reasons. In the first place, they imply that an indefinite condition as regards the insulation of the circuit is preferable to a clean-cut, definite condition. Circuits supposably insulated may or may not really be so, and as a matter of fact the full stress may be, and frequently is, on one side of the circuit; the very condition that the underwriters refuse to sanction is continually occurring without such sanction.

The Association of Edison Illuminating Companies has had a Committee on Grounding the Neutral since 1890. The recommendations of this Committee being based on extensive experience are entitled to great weight. Their recommendations have uniformly been that the greatest safety was assured by the practice of grounding the neutral. This is the practice of nearly all the large Edison companies, as is well known. In some cases, the companies have been forced to this position because they were not able to free the neutral from grounds; in other cases they have deliberately adopted this as the best remedy for many troubles due to operation, to dangers from fire, and to abnormal pressures.

This Committee has claimed at all times that grounding the neutral has the advantage of "Reduced fire risk, since a ground created inside a building will, if on the neutral conductor, be in no danger, while if on the outside conductor, it will probably fuse a safety catch, or do all the damage of which it is capable at once, and not remain inactive until the appearance of a subsequent ground on the opposite side of the system, at a time, perhaps, when no person may be at home."

"Also, that the electrical pressure capable of causing damage by reason of any ground established, either in tubes or in buildings, is limited to 120 volts, whereas the active pressure might otherwise be 240 volts; and for overhead circuits greater immunity from lightning."

The disadvantages reported by this Committee are possible troubles in operation, particularly in regulation and excessive registration on the meters, but no increased risks are foreseen. The Committee has made its reports in face of the fact that the underwriters have uniformly refused to countenance the practice.

It is a notorious fact that the systems in nearly all of the large cities, particularly the Edison systems, are grounded, the neutrals in most cases being permanently grounded at the junction boxes. It is equally well known that in several of the large alternating current distributing systems, the neutral wires are grounded. The authorities of the Board of Underwriters certainly should, and probably do, know these facts. To keep rules in print prohibiting such practice under the circumstances, must weaken their authority, and lessen the respect for all their rules.

The rule against grounding the circuit is not the only one that is continually violated, and by inference with the knowledge of the Board of Underwriters. The second is that which calls for double insulation between primary circuits carrying 6,000 volts or more, and secondary house-wiring systems. I protested against this rule when it was promulgated, citing cases where it was violated at that time, and saying that it would without doubt continue to be violated. It is violated in every large city employing alternating current distribution with synchronous converters feeding a direct-current distributing system, as for instance, in Brooklyn, New York and Boston, not to go further. Such a rule is vicious in its tendences, and should unquestionably be modified.

In this discussion alternating and direct-current systems are considered subject to the same risks; this is the case now in the large cities where synchronous converters are used; but in any event, the arguments outlined will apply to any system subject to an abnormal potential, whatever may be the cause.

This is a matter that concerns every electrical manufacturing company and engineer in the country; this INSTITUTE has recently taken part in the formation and modification of rules governing electric installations and has endorsed the National Electrical Code. Therefore, it is entirely in the province of this body to make recommendations as forcibly as it may see fit to the Board of Underwriters. I therefore offer the following resolution :

Resolved, that the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS favors a rule permitting the grounding of one wire of every low potential consumption system.

Resolved, that the Committee of the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS on the National Electrical Code is hereby requested to confer with the Electrical Committee of the *Underwriters' National Electric Association* in relation to recommending to the *National Board of Fire Underwriters* the passage of a rule permitting or requiring the permanent grounding of one wire of such systems, under suitable restrictions.

Resolved, that they also confer with the same committee in regard to recommending to the National Board of Fire Underwriters some modification of Rule 39 of the Code in relation to the installation of secondary wires when the primary wires carry a current of over 3500 volts potential.

Discussion.

Prof. Elihu Thomson:—I think it proper I should lead off in this discussion. In this connection I wish to offer a moderate criticism as to the use of my name in regard only to the film cut out. The fact is my name should be used if at all on the three devices together, as there appear in patents of the United States back about fifteen years ago, the grounded secondary and grounded shield, and the devices which respond to high potential, and work cut-offs. These arose from a feeling I had that no system could be considered fit for use, unless it had some such device. In fact, if the files of correspondence in our records could be looked up it would be found that upon every occasion that arose I took the opportunity to insist that alternating systems, to be considered safe, must be protected in some such way, and I felt that alternating currents should not be put into service until we had got some such safety device. Just as soon as we got those devices we were ready to do business. Now, the actual state of the case is this, that the wiring in my former house in Lynn, I think the plant was installed about 1887, had its secondary grounded. I grounded on gas and water pipes and preferred that arrangement to any other. But there were kickers as there always are against such simple proposals and as a compromise we afterward did not ground the secondary completely, but put a film between the secondary and the ground. That was the origin of the film cut-out. We put the film on both legs of the secondary, and of course a primary connection was supposed to break it down and give us protection when needed.

I will say in my own plant I also used a film after the first grounding in the following way. I felt that something might occur to interrupt the connection with the leg of the secondary on which the ground was, and therefore to be perfectly safe about it I put a film between that leg and the other so that if anything interrupted the ground side, the film would break down and ground the other side. This is all of course ancient history, and I have been doing my best in correspondence and in papers before the *National Electric Light Association* and upon similar occasions to enforce just what is put forward so nicely in the paper before you. I endorse everything that is said, and I hope some action will be taken by a body so influential as this, toward getting some such thing adopted.

Mr. C. M. Goddard:—Mr. President, I did not understand that Prof. Thomson formally seconded Dr. Hutchinson's resolution. I should like to second the resolution and for several reasons. Not, however, because the underwriters are fully convinced as yet, that one of their rules, almost as unchangeable as the laws of the Medes and Persians, that no circuit should be permanently grounded, has been wrong all the time, I am not convinced of that fact, but because we are delighted to have the INSTITUTE take up any subject in relation to our rules and try and help us to arrive at a right solution. I think in the past the INSTITUTE has been almost too much afraid to put itself on record as favoring or opposing any particular rule. Individual members have frequently spoken (sometimes possibly in disrespect) of the underwriters' rules *in general*, but to come out fairly and frankly and try and help us to improve the rules has been a course which they have not taken so freely until through the efforts of the National Conference we were brought more closely together, and found we were not on either side quite such dreadful creatures as we had supposed, and that we were both trying so far as we could to work toward the same end. The station manager is just as much interested from a commercial standpoint, that his current shall not set fire, as the underwriters are.

I think it very probable that this grounding of the secondary may be the best solution, but the underwriters are always conservative in such matters. We had the various earthing devices. I have seen the cut-out of which Prof. Thomson speaks, and I think I have heard it spoken of by station managers even more disrespectfully than the rules. These devices have given trouble, they were steps towards something which we were trying to accomplish, but they were not perfect solutions. We have found many of these film cut-outs supplied with pieces of straw paper and writing paper and even blotting paper, one time we found two thicknesses of blotting paper, and should not have been surprised to find asbestos board used.

But the great point in favor of grounding a circuit permanently, is that it would be thoroughly and properly grounded, and as Dr. Hutchinson says you then know what you have. It is in my mind largely a question of choice between two evils. I don't think that anybody will deny that the grounding of one side does to a certain extent increase the fire risk, especially where we find combination fixtures, and the wires run in proximity to gas pipes, and in a great many of our older installations I should be afraid to ground either side permanently until we had made careful tests. You are all familiar with the old style of insulating joints, lots of them in existence to-day, and underwriters' wire, wooden cleats, no insulating tubes where the wires pass through the building. When you came to a brick or plaster wall, eight or ten years ago, there was a piece of soft rubber tubing there; what it is now you can all imagine. The underwriters are conservative, but they are delighted to have the Institute help them, and if these resolutions pass, and the Committee on Code of the Institute will take this subject up they will find that the Electrical Committee of our Underwriters' Association will take it up just as carefully, and try to be reasonable, and I have very little doubt but what we shall arrive at a solution satisfactory to both sides. The same way with the rule in regard to double transformation with extra high potential. If you undertake to carry 10,000 volts through the streets of Boston we (the underwriters) can't stop it, but we say there must be a double transformation before it is put into use in insured buildings, and the chances are that this will largely relieve the streets of these extra high potential wires through the use of transformer stations in the outskirts of the city. The presence of high potential wires in city streets is certainly something that firemen very strongly object to at the time of a fire, and if the underwriters did not impose any restrictions, the city authorities probably would. We would be delighted to have you help us in making our rules. We modified this rule last year at the request of the Institute, and we will certainly modify it further if you will only come to us and show us that you are right, for we have a great deal of respect for the opinions of the Institute. We now have six months to work on these questions before the next meeting of the Electrical Committee of the Underwriters' National Electric Association, and I have not the slightest doubt that if your Committee will do their share of the work we will arrive at a solution that will be satisfactory all round, and I hope the Institute every time it finds a rule criticised will get some member to write a paper and give us the expression of their opinion. And I most heartily second Dr. Hutchinson's resolutions.

Prof. Goldsborough:—After the opinions that have been expressed in favor of grounding the secondary, I hardly feel like saying anything in opposition, but I am very much interested in the subject and all my experience has tended to lead me to feel it is not best to ground the secondary. I do believe that grounding the secondary is much better than any other device that has been suggested, outside, possibly, of the shield. But I think the grounding of the secondary is a step in the wrong direction. It does not accord with present tendencies, nor with the rules that the Institute has adopted requiring very rigid tests of all machinery and defining these tests very carefully. In other words, if you ground the secondary you give people who are installing inferior equipments a loophole through which they can crawl. An electrical installation will not be any stronger than its weakest point, as in the case of any other installation of an engineering character, and I think what we should do would be to advise that after an installation has been made it be subjected to a two or three or four hour semi-short-circuited test, *i.e.*, a test of several hour's duration with 50 to 100% more current flowing through the mains than they are designed for. In this way all the joints, wiring and connections will be subjected to a greater temperature rise than when in service. After this has been done, all of the short-circuit connections should be broken that have been put in, and the system should be connected on to a high pressure transformer through very light fuses. By increasing the voltage on the whole system until double the primary voltage has been reached, the system will be subjected to a sufficiently severe test. If the contractor has to put in a job with those conditions staring him in the face he will be careful about the details of the installation. Ordinary insulation installed as we do it to-day will stand such a test if it is carefully put in. The fires we have usually result from there being a weak point, some point where a workman was careless in putting material in place. In view of present method, I think any engineer in going over a large installation or even a small one, will be more than likely to find just such weak points.

I don't know but that this statement of my opinion on this matter may seem rather radical to some of the members of the INSTITUTE and not wholly warranted. It is a sort of departure. But if we demand that a transformer shall be built to stand a test at 10,000 volts it does not seem to me we should put in a very low insulation resistance in the secondary equipment. I don't refer to light insulation, but insulation installed in a careless way. Furthermore, I am quite sure that members of the INSTITUTE will bear me out in saying that the underwriters experience most of their troubles from not seeing that their rules are carried out. I have in mind an installation where I have requested them to send an inspector, and that installation has been in a year and a half without any attention on their part. They have taken my word for it that it is all right. The building in which the installation was made is valued at possibly $20,000, and if there is a fire there of course their policies will hold.

I would like to say in conclusion that in experimenting, as far as the element of personal care in such matters is a factor I think we all formulate a set of rules for ourselves as we gradually become accustomed to high-voltage apparatus. We learn automatically to protect ourselves. If I am using voltages as high as 10,000 volts I don't want anything grounded. I would like rather to have the transformer and generator and apparatus insulated, and personally, I feel safer under those conditions.

MR. STEINMETZ:—I beg to disagree herein with Prof. Goldsborough. Regarding the proposed test of operating the lines with 50 to 100% more current, I do not see any object, since the mains should be protected by fuses against overheating, and it is the business of the underwriters to see that this is done.

Operating at twice the voltage does not seem to me to be quite sufficient protection either.

PROF. GOLDSBOROUGH:—I mean twice the high-tension voltage.

MR. STEINMETZ:—Twice the high-tension voltage as a rule appears to me an unnecessarily severe test between the conductors, since a breakdown between the conductors short-circuits the secondary system and thereby blows the fuses. The real danger appears to me to lie in the insulation against grounds, and no specifications regarding this insulation appear sufficient if there are no means available for testing the insulation.

But if, as Dr. Hutchinson proposes, it would be made permissible to ground the secondary circuit, or as I should rather like to see it, mandatory, then it would be easy to test the dielectric strength of the secondary system against ground, and such a test, which can be made in a few seconds, could be prescribed before accepting an installation.

By opening the ground wire and inserting in the gap the high-tension coil of a small testing transformer of 1:10 ratio, with its low tension coils connected to the secondary mains, the whole secondary circuit is raised to a potential against ground equal to ten times the operating potential of the secondary circuit. As you see, the existence of a permanent ground would make such a test very easy indeed and feasible in a few seconds, so that the installation could even be re-tested from time to time. I need not say that I am very much in favor of Dr. Hutchinson's resolution.

There has been in the last few years a great deal of discussion as to whether grounding the secondary is safer regarding fire risk or not, and the majority of opinions seem to be strongly in favor of grounding.

Regarding danger to life, however, there never has been any doubt, and never can be, that the only absolute and perfect protection against danger to life in case of an accidental contact of the lighting circuit with a high-tension circuit of any description, is a permanent and absolute ground of the low-tension circuit.

I should certainly hesitate, when standing for instance on the damp floor of a cellar, to turn on an incandescent lamp when knowing that the only protection against certain death is the insulation of some unknown type of transformer which hangs somewhere on a pole, exposed to the weather for years. The film cut-out as a protective device is certainly better than nothing, and there may be people who would enjoy getting into a 3,000-volt circuit if they know that a film cut-out will probably open the circuit quickly, but I myself should decline to do so. A further feature in direct current systems is, that an extended ground of the neutral offers the best protection against electrolytic action by restricting the path of the leakage currents from the unavoidable leaks on the outside wires.

PROF. GOLDSBOROUGH :—I forgot in mentioning the test I advocate, to say that I also favor the test between the secondary mains and the ground. My idea is to have a test which will be analagous to the test to which the transformer itself is subjected. As regards the matter of using double current in making the test, that is simply for the purpose of heating and thereby weakening any point that may be faulty so that when the high voltage test is applied the fault will break down.

DR. BELL :—I would like to call the attention of the INSTITUTE to the fact that it is not the initial condition of the circuit that makes trouble. Prof. Goldsborough says tests must be applied with the utmost rigor. Where they are running in adulterated rubber on us you cannot expect long life of insulation, and that is the trouble with any position whatever depending upon strict insulation tests at the initiation of a plant, be it a large plant or be it house wiring. Two or three years after the plant has been in service it is just as likely to be full of leaks as if it had been so in the first place. Test your circuit as thoroughly as you please to satisfy yourself that on this 27th day of June it is all right but ground it so that on the 27th of next June it will be safe even if the insulation is deterioriated.

One of the things that engineers forget is the danger from arc circuits. Those are generally run in a slipshod manner, the insulation may be hanging from them in strings, and as to the voltage, it is all very well for the underwriters to demand double transformation for anything over 3,500 volts, but how about the arc light circuits which are running anywhere from 2,000 to 5 or 6 or 7,000 volts over all sorts of overhead wires. The Edison circuits are in tenfold more danger from arc light circuits than they are from any breakdown between the primaries and secondaries of transformers on their own systems and it is in respect to all these contingencies that we must guide our practice. If we ground the secondaries we are compelled to be safe everywhere. The more thorough tests we put on prior to that the better. That matter will take care of itself. But once grounded you definitely limit the potential between the wire and the ground. And there is another thing in this connection which comes into practice in some of our high-tension long-distance plants. Supposing you were using a three phase transmission circuit as is generally the case in high-voltage plants. How about grounding the neutral point of the primary? How about grounding the neutral point of the distributing primary? By doing that, you very greatly reduce the tension between wire and ground, and consequently increase your safety all round. The only legitimate objection that can be brought against the grounding of secondaries and grounding of primaries in that way, is that objection which may be brought by the manufacturer on the ground that it is subjecting his machines to outrageous strains. But since the manufacturer is perfectly willing to take his medicine, and would prefer to have the machines grounded, I think it is very far from the province of those of us who have nothing at stake in that matter to say "You shan't ground your secondaries because something that we do not understand may possibly cause trouble of a character we cannot predict in some wiring we know nothing about." And I can see from my own observation that the application of half the underwriters' rules in full rigidity would do more to secure safe wiring than the publishing of a volume of instructions and addenda thereto. I have seen houses wired within a thousand feet of the habitual residence of men whose business it was to inspect, so dangerously that I would not care to take electric light if I lived in them. It is the carrying out of a few simple rules to their legitimate conclusion that will give us safety. Once your circuits on the secondary side are grounded you know where to to look for trouble. You must keep up the insulation of the circuit with respect to ground and that can be done. Once that is done we are on comparatively safe ground. And in seconding again Dr. Hutchinson's resolution with respect to bringing this matter before the underwriters I would like to call the attention of the INSTITUTE to the fact that it is probably known to most of us that in making this provision for grounding neutral wires and grounding secondaries we are simply following out a line which they are following abroad to-day continuously. Not only is it permitted but it is required in the installation rules abroad, and I would like Mr. Steinmetz to go considerably forward, and not only make the grounding of the neutral permissive, but mandatory: grounded at some point which will protect the secondary system from the high-voltage current.

MR. JAMES I. AYER:—This discussion is agreeable to some of us. Some ten years ago, realizing the necessity for this protection I secured a compromise with the underwriters by having them permit me to construct and use circuit breakers with magnets of high resistance; these magnets were connected across the secondary mains and a connection to ground made between the two magnets, and in this way we grounded the secondaries. Anything over 400 or 500 volts coming in on the secondaries would pass sufficient current through either or both magnets to open the main line switch. The whole device was enclosed in a box so that it was impossible to close the switch when once opened, without removing the cover of the box that was closed with a seal. I got permission to use such a device as this but could not get permission to ground the secondary.

Some four years ago, Prof. Puffer, at a meeting of the INSTITUTE brought up this subject and strange to say there was no material discussion. He brought it up with fear and trembling and said he was going to ask for the consideration of a thing which would lead to much criticism, namely, the question of grounding secondaries of alternating circuits and particularly those operated from high-tension, long-distance transmission mains. The members of the INSTITUTE at this meeting, four years ago, seemed to have no appreciation of the necessity of this or some similar protective method. To-day the necessity is not as great as it was ten years ago, nor even fifteen years ago when Prof. Thomson realized it, although at that time there were very few circuits to protect.

I desire to-day to add my testimony to others here, indicating the necessity of this action being taken because I think it is a part of the work of the INSTITUTE at its meetings to do just this sort of thing and I trust the resolution will get the full endorsement of all the members present.

There is one point in connection with all this, the detail of grounding and the detail of protection as Dr. Bell has mentioned is a thing to be looked after. Many methods have had to be abandoned in the past on account of the difficulty of securing safe and permanent grounds. That is an element which must be considered in determining any final resolutions in connection with it because I take it, it is necessary to prescribe some methods in detail as to how the work shall be carried out as well as to indicate generally that it should be done.

CAPT. WM. BROPHY:—I am somewhat familiar with this subject, having discussed it with Prof. Thomson several years ago. I did not agree with him at that time that it was a good plan to ground the secondary wires, for the reason that we did not have at that time the improved devices that we have to-day. We had then insulating joints that were such in name only. We had soft rubber tubing and various devices that have long since disappeared. Again I think that to the fact that this plan was opposed at that time, may be due the great improvements that have been made in transformers and better forms of insulation and translating devices generally. The only safe rule to follow in the transmission of electrical energy is to construct the circuits in the best possible manner, use the best form of insulation and the best devices that can be procured, in fact have every inch of the circuits as nearly perfect as they can be made. We now have transformers that are far superior to the earlier types, and which are very nearly perfect. With the great improvement in material and devices, I can see no great objection to this plan.

In New England there have been but two lives lost since the introduction of the alternating system, due to the breaking down of the insulation of transformers, one in Boston, the other in Woburn. There has been some criticism of the underwriters indulged in which I think is unjust. The station manager is apt to throw the burden that belongs to himself onto the shoulders of the underwriters' inspectors. He owes it to his company and to its patrons to see that the most approved form of electrical work is done, and the best material used.

He injures the business not only of his company but of every other one throughout the country when he countenances slipshod methods of electrical construction. It is true that the manager is not always responsible for fires due to the failure of insulation and faulty construction, while we have that peculiar genius —the would-be contractor. Most of the contractors are reputable gentlemen who try to do what is right unless driven to do otherwise by irresponsible parties and unfair competition. Now the underwriters cannot undertake to supervise every installation that exists throughout the country as rigidly as they should do. It is too much to ask or expect of them. They have a right to depend largely on you and you should assist them; I mean those in the electric lighting business. They should put their shoulders to the wheel and do the greatest amount of that work, and not throw the entire burden on the underwriters and municipal inspection boards. As I have said before, I think the time has come when this plan of grounding the secondaries can be adopted, except in the case of some of the older installations the conditions of which we know so little to-day.

Ten days ago in this city we had a breakdown of a transformer in what is called a fire-proof building. In it were the old type of insulating joints which proved to be of little value; the current passed over them without difficulty and a fire occurred at every fixture. Had it been a frame building it would have been destroyed. The wires were run in paper conduits, on which time had laid a heavy hand, resulting in grounding the system, to which was due the failure of the transformer. It may be asked where were the fuses? If there is any one thing that is more unreliable than the ordinary fuse I have yet to see it. In this case the fuses failed to "fuse;" they were a first-rate fuse from one point of view, their melting point being exceedingly high and they did not give way and open the circuit; as heat resisters they proved a success.

The neutral wire has been grounded here in Boston for several years. Why shouldn't it be? You cannot keep a system as extensive as that of the Edison Electric Illuminating Company clear of grounds. While we can maintain a high standard of insulation in the interior of buildings, we find no serious trouble resulting from the grounding of the neutral wire. The true rule to follow, is to use the best possible insulation and do the highest class of work In the underground system of this city, which is limited to the section south of Dover street we can maintain a maximum of insulation, but with very few exceptions a portion of the circuits extend beyond the underground district, and at times in the overhead portion, there is absolutely no insulation, consequently the absolute resistance of the circuits is greatly reduced. Owing to this fact, the insulation of interior circuits should be, in fact must be, maintained at the highest point possible.

PROF. PUFFER:—I can speak of this matter from three points of view: first, as the teacher on electrical engineering; second, as a member of the INSTITUTE; third, as consulting Electrical Engineer of the Associated Factory Mutual Insurance Companies.

As the general tendency just now seems to be for some reason to attack the insurance interests for failure to require what less than a year ago was bitterly opposed by these same gentlemen, I will speak as an expert of the insurance companies. On taking up the matter of rules and regulations, I found that things were in a very bad shape, especially the alternating current installation.

In making up our minds what to do for rules, it seemed best to adhere to the existing rules at first, and while looking around for means of increasing the safety of transformer work I consulted about all of the electrical people I happened to know, and they were a great many, and I am glad to say that two of the members who have spoken here, Prof. Thomson and Mr. Steinmetz agreed thoroughly at that time that there was no known device, no known trick of engineering that they knew anything about which rendered secondary wiring safe except the grounding of the neutral of the secondary. But business matters were such that it didn't seem desirable for every one to take that view, yet I always found that even those men whose official views were against grounding transformers had a sort of sneaking impression that when the transformer was outside of their house it was going to be grounded, which I always considered as expressing their positive feeling that there was a decided danger.

When I brought up the plan of grounding the secondary, at a meeting of the INSTITUTE in New York a few years ago, I found that the temperature of the meeting was much below that of liquid air and the plan was then treated as vicious.

I want to go on record as stating what I believe to be the truth, that this grounding of the secondary has been held off for two years by the action of gentlemen in connection with electrical industries. I have attended the annual meetings of the insurance inspectors and I found two years ago, that it seemed as if we could adopt a rule requiring, under suitable conditions, a grounding of the neutral circuit, but it was prevented by the statements and efforts of certain electrical gentlemen who I presume believed what they said. Now, it is unfair to state that the majority there at that time were responsible for failure to permit the grounding of the secondary under suitable conditions, which was permissive according to our rules. At the Convention last December, action was deliberately prevented in substantially the same way, again relieving the insurance people. It is not fair for any man to turn around and say that this thing has been held off as long as it has purely by the insurance people themselves. It is not so.

There are however a number of things to bear carefully in mind in connection with this grounding of the neutral or middle point (as I prefer to call it) of the transformer itself. It has been stated here by some gentlemen that the ground on the middle of the transformer secondary removed all possible danger. That is a mistake, unless the gentleman couples with it the fact that the transformer is provided with reliable fuses in the primary wires. For if it is not fused you can then get a drop through the impedance of the secondary that will give you a dangerous difference of potential between the parts of the secondary system and the earth.

Another thing in connection with this ground wire is that it should be sufficiently large to have a greater carrying capacity than either wire of the transformer itself. I think you will see from this consideration, that all such things as earthing devices become simply negligible, they disappear in smoke with the first short-circuit that happens to come.

In regard to grounding the secondaries of transformers there was one criticism brought up that led me to think that the gentleman had in mind the possibility of the ground being removed by the blowing of the fuse, leaving the transformer still in, and that there could be no installation test made of the wiring under such a state of affairs. If you ground the middle of the secondary, the simple removal of the ordinary prescribed fuse leaves your wire absolutely free for any test you desire and the ground is where it should be, back of all fuses, on the secondary side, and protected by the fuses of the primary side.

I want to call your attention to the fact that the presence of a large grounded system may in itself introduce a dangerous element from the fact that it as a whole becomes a conducting system for foreign systems, and I refer to the fact that a large grounded system may become a return for railway circuits to such an extent that the regulation of the two sides of a three-wire system will be found to be complete.

I would carry grounding to such an extent that I would compel the absolute grounding immediately at point of entrance into buildings of every system (except series arcs which ought not to be allowed in buildings) and including such things as water pipes and gas pipes. Not many hundred feet from where we are now there has been a serious case in which a building with no electric wires in it was set on fire by a current which came in over a water pipe and went out by a gas pipe. If these pipes were connected in the basement and therefore grounded at same point and that building had electric wires and the neutral were grounded, nothing would enter that building of a greater potential than that allowed by the rules, of 300 volts.

MR. STEINMETZ:—In connection with that most formidable and least tangible danger, the contractor who does improper wiring, I would call attention to the advantage of the grounded circuit in permitting a quick and simple test of the insulation strength.

It is true that the methods of insulation have been essentially improved during the last year and as a minimum test of insulation in transformers now 10,000 volts is proposed. But nevertheless, a transformer installed to-day and tested for 10,000 volts may be broken down absolutely to-morrow by causes beyond your control and unavoidable, as a secondary lightning stroke.

The grounded shield is a fair protection also in the transformer, but to be perfectly safe it must hermetically enclose the secondary coil and all its leads, which is not well feasible, and must be perfectly grounded which is very difficult to accomplish at the place where transformers are usually located.

PROF. THOMSON:—On the point of the difficulty of making a ground I see that sometimes lightning makes a mistake in that particular. At Lynn a while ago it thought it had a good ground by striking the water pipe but it had to strike two or three houses and had to break about a dozen joints in water pipes to get away to earth.

PROF. GOLDSBOROUGH:—I think probably by taking the position I have in this discussion there has been brought out the fact that after all the difference between the ideas I have and those voiced by the other speakers is very slight. I think it has been very strongly brought out so far, that even though the secondary is grounded, as I am quite sure from the sentiment generally expressed it is going to be, we must insist upon good insulation of the house wiring. In opposing the ground I do so largely because I think it will lead to people feeling that, as they have a ground protection, they are safe anyway, and they will be more or less careless about the matter of insulation. In fact I know of a case in which weather-proof wire was used in interior wiring and no complaint has been made about it.

The old transformers which are now in use should not be subjected to the strain necessarily resulting from grounding the secondary. The plan which is suggested here might well be made to apply to future installations. We can't, unless radical steps are taken, make existing lighting plants throw out their old transformers. Suppose we make a ground on the water and gas mains in an installation where one of the old type of transformers is in use. The result will probably be that discharges, either direct or induced on the line, will break down the insulation between the primary and secondary of the transformer and then take direct passage to the house. To my mind, all grounding should be outside of the buildings and the grounds should be well made. It is not always easy to secure a good ground and even where care is taken in the course of a year or two you have to go over the work and readjust the ground connections. If it is not made on the water pipes and gas pipes you have to go to considerable expense in making one that is satisfactory.

I therefore urge should the INSTITUTE *recommend that secondaries be grounded, that it also recommend that an insulation test be required wherever a ground is made.*

MR. GODDARD:—I take it the idea of Dr. Hutchinson is that the INSTITUTE should express itself as to whether it is advisable to permit the grounding of one wire. Then he goes on in his resolution authorizing the Committee on Code to confer with a similar committee from the underwriters as to details, and it will be noticed that the second resolution reads "the passage of a rule *permitting* or *requiring*," if it should be the result of that conference that the general opinion was that we should require the grounding, that might be the recommendation. It goes without saying that you want a good ground and a permanent ground. And that is the reason we want this conference to settle the details of any rule we may recommend. But they can't be settled here to-day.

MR. C. W. RICE:—The largest lighting interests endorse this resolution. In New York and Brooklyn the system is now operating with from 6000 to 7000 volts on the primary, and it would be a great advantage to all those interests if this change in the rules were made.

MR. HAMMER:—I think if the literature of the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS, the *National Electric Light Association*, the *National Conference on Standard Electrical Rules*, and the *Underwriters' National Electric Association* were gone through you would find this subject has been given a good deal of attention heretofore. It has been expressed by gentlemen here present that the principal difficulty was that the transforming apparatus had not arrived at that condition of perfection which would warrant such action being taken, and if it were held off the manufacturers might perfect the apparatus or find a satisfactory method of meeting the difficulty. At the time of the National Conference meeting I believe the best thing recommended was an automatic device which would open the primary circuit when the potential in the secondary rose above a certain value. Prof. Puffer spoke of this subject being coldly received at the INSTITUTE meeting in New York City. There has not been an engineer who would give a decided expression of opinion on this point and back it up with the results of experiments which would warrant such an adoption. The question is an old one and I think the underwriters and certainly the National Conference at the time of their meetings thought the grounding of the secondary a thing which would eventually be done though it was hoped the manufacturers would submit something that would take the place of that. At any rate, the time had not then arrived.

PRESIDENT KENNELLY:—Before putting the question I may be permitted perhaps to make a few closing remarks. This is an old question. It has long been in existence and long been a matter giving rise to much difference of opinion, but especially between the underwriters and electrical people.

The facts are, as we know, that every low-tension system to-day, of any kind, whether alternating or direct, when normally insulated, has its central point at the potential of the earth, and, therefore, so long as a low-pressure system is properly insulated it is normally subjected to the same conditions of pressure, the same breaking-down conditions, as would exist if a central point of that system were grounded directly, and only when a loss of insulation occurs on either side is that normal condition upset or modified. Moreover when a ground is made on either side, the conditions tending to produce accidents are worse than when the neutral or middle point of that system is permanently grounded.

As regards the argument that if the secondary circuits of transformers built in the past had been grounded at some point, they would have broken down, is that not a very weak position to take in regard to the security of life and property to-day, when if a high-pressure circuit becomes crossed with such a circuit we are likely to produce that very condition of breakdown. Is it better to lose a few transformers of weak insulation by grounding their secondaries, or to run the risk of accident from a high-pressure cross?

I do not know any man, conversant with the facts, who would let any person for whose life or safety he was responsible go into a damp cellar and touch any part of a secondary circuit when there was a high pressure turned on on the primary side. I want to see the man who would permit any child of his to do that thing unless adequate precautions were taken for protecting the secondary circuit.

The same is true in a modified form in regard to any low-tension system overhead or underground, Edison, direct or alternating. It is no argument whatever to say that owing to transformers being weak in the past this grounding of the neutral was objected to, because the way to make transformers better was to make manufacturers ground their secondaries. That would necessarily throw the full normal stress on the transformers and force the manufacturers to make the transformers capable of standing this full normal stress.

[After the presentation of certain amendments, the resolutions offered by Dr. Hutchinson were passed in the form in which they now appear on page 436 and the session of June 27th then adjourned.]

A paper presented at the 16th General Meeting of the American Institute of Electrical Engineers, Boston, June 28th, 1899, President Kennelly in the Chair.

PLEA FOR A WORKING STANDARD OF LIGHT.

BY S. EVERETT DOANE.

It was my desire to present what I am about to say as a part of the discussion on the report of the committee on the standard of light. As this committee will not report at this meeting, I bring the following before the INSTITUTE with no intention, of course, of interfering with the work our committee is doing. We all agree as to the importance of this work. What I wish to propose is that this committee or a new one, fix definitely the means for maintaining a working standard of light pending the adoption of a primary one.

There are scattered over the country many well equipped laboratories, and in the near future every central station and large isolated plant will be provided with means for measuring light. There is not now in existence any workable primary standard which can be used by the ordinary photometer corps.

It may be said that this is equally true of all primary standards. The mercury column or resistance standard, in fact presents so many difficulties that it is rarely ever used, even in our best equipped laboratories. In reply we can say that the standard of light differs from all these in that its terms are so indefinite and so crudely outlined that two laboratories cannot produce the same value for the candle power, if they work entirely independently. The discussion on this paper will probably bring out very diverse views as to what is the proper direction in which to work to eliminate our present difficulties with the light standard.

The standard of light is in quite another class from the other standards. Every instrument maker is anxious that his instruments shall be calibrated in true units and he can so calibrate them if he uses care. He sends his instruments out so calibrated to the best of his knowledge.

A lamp user may say to the manufacturer, "Your standard is wrong," and neither ever dreams of suggesting a recalibration; they know by experience that an agreement cannot be reached. No observer can even duplicate his own work. What is the result? Every lamp maker in the country is sending out some lamps which are wrongly marked according to his own standards because some of his customers have standards of their own.

Lamp makers differ among themselves and there exists no way by which they can be brought together. It is obviously to the interest of the whole electrical engineering fraternity that some action looking to the correction of this condition be inaugurated.

In passing I wish to go on record as saying that in a very few years the incandescent lamp will be adopted as a primary standard. It could be so adopted to-day if lamp makers would only tell what they know.

Every committee (our committee included) ever having to do with investigation of proposed light standards has found an incandescent electric lamp a very convenient secondary standard. It has been entirely satisfactory, so much so that my whole purpose in writing this paper has been to see if we cannot inaugurate some action which will result in the appointment of a committee who will provide means whereby a standard of light may be arbitrarily maintained. This committee could send out under proper restrictions properly seasoned and calibrated incandescent lamps for this purpose.

It does not matter just how this is done. Possibly a national laboratory could be founded. A committee of the professors of electrical engineering from our various colleges might agree to do this for us. Something should be done, and the first step toward it will be the appointment of an INSTITUTE committee on the maintenance of what our committee on light may decide is a proper standard value. Such a decision will be arbitrary and may have to be changed later but what of that? We need now a common working standard of light.

That the discussion may be entirely free I refrain from making any definite suggestions until the views of the members may be obtained, but whatever these views may prove to be, I hope some action will be taken which will afford us temporary relief, and enable our committee on light standard to pursue its work unhindered by the necessity for hasty action. We can take action none too soon. A year from now, at the rate photometers are being made, we shall have as many standards of light as Joseph's coat had colors, unless something be done.

DISCUSSION.

MR. STEINMETZ:—I think the matter brought here to our consideration is of considerable importance and for this reason I should hesitate to take any immediate and rash action.

I believe some years ago a committee was appointed to investigate photometric standards, and the committee made a preliminary report but has not yet concluded its work. I believe the committee was investigating the acetylene standard. Acetylene appears to me to offer unusual advantages as a standard of light since it can easily be produced in chemical purity from copper acetylide Cu_2C_2, and its light has a perfectly white color equally suitable for incandescent and arc light photometry. The amyl-acetate standard while by far the best available at present, has a number of serious disadvantages, being somewhat inconvenient to handle, and especially differing in color by its reddish light considerably from the color aimed at in artificial illumination.

Regarding absolute measurements of light, I see no reason why this should not be possible. It is true that light is not a physical quantity in the usual sense, since as light is understood the radiating energy in that range or wave lengths where the radiations are visible to the human eye. A photographic plate for instance, "sees" another and a bolometer still a different range of ether waves. Still, however, it should be possible to define as the total light intensity the energy of the ether waves within the wave lengths from 3,900 to 7,000 which is about the visible range.

MR. CHARLES P. MATTHEWS:—I might say a word about the committee which Mr. Steinmetz refers to. Some four or five years ago a committee was appointed of which Dr. Nichols was chairman and of which I had the honor to be a member. We made a report of some forty or fifty pages, the object of which was to show how poor the standards were. We succeeded in accomplishing that. Since that time we have not done anything collectively as a committee, although we have done something individually. Dr. Nichols is at work on some standards. I hope to see recommended in the near future a standard that will be of some service. I am confident that it is not a problem that is beyond solution. At present the best standard in my opinion is the amyl-acetate standard, in opposition to Mr. Steinmetz's opinion, because it is the steadiest standard. It is the most independent of the quality of the material burned—that is you can burn it with a considerable degree of impurity and still get the same intensity of light, and it is reproducible in a high degree. The qualities wanted are steadiness and reproducibility. That is, you want in Boston and New York and San Francisco a standard which will come up to the specifications without having a necessity for a central bureau. First, steadiness and second reproducibility, third, quality. That brings in a point of importance. It is clear that the arc light differs so greatly in quality from the incandescent lamp that we cannot employ the same standard for the two. If we wish to measure the arc lamp to-day it is necessary to proceed stepwise. By so doing you can eventually reach a standard which has approximately the same color as the arc and you reduce the error to a minimum. I think there is no reason why we should attempt to measure arc lights by the same standard as glow lamps. I am quite in sympathy with Blondel of Paris who has suggested an arc light standard, a standard obtained by screening off a portion of the crater of the arc. That to be used solely for arc light. Unfortunately it has been shown that the intensity of such a standard is not entirely independent of the quality of the carbons used, but I am in sympathy with the principle there that we should have a standard for arc light photometry. I think in conclusion we may well adopt a provisional standard. I know of no better standard than the amyl-acetate. We may indicate the character of the fluctuations which are due to the amyl-acetate standard in comparison with the others. If you take the amyl-acetate standard and set it up before a bolometer in the manner done by your committee you get a record which shows the steadiness of that standard. The records of Mr. Sharp and Mr. Turnbull which were put in that report I refer to, are something like this in a very crude way. That shows the reproducibility of the standard at any rate. You are able to get the same thing at different times. Of all the standards tested by the committee, those fluctuations were the smallest, the steadiness was the best. Therefore as a temporary standard I don't think we can do better than to accept the amyl-acetate standard.

The President:—It may assist the deliberations to state the facts in a small compass. The Institute appointed some time ago a committee to report upon the question of a luminous standard and the means for obtaining measurements from the same. That committee has reported that the amyl-acetate lamp is the best standard available to day, and that in using this, certain precautions should be taken in comparing incandescent lamps. Mr. Doane's suggestion is that the primary standard is of very little practical importance, and that what is wanted is a stock of secondary standard incandescent lamps which have been standardized by some recognized authority.

Prof. Goldsborough:—I think at the present time we are in a position to look to the organization in this country of a standard laboratory and to my mind the Institute could not engage in a more profitable undertaking than that of looking into this matter, and seeing if it is not possible either for the INSTITUTE alone or for the INSTITUTE in connection with other societies in America, such as the *National Electric Light Association* and the Fire Underwriters, or for the Government, to establish such a laboratory, that we may have a recognized laboratory, where we can send both lamps and instruments and other things and feel that they will receive proper attention in the hands of unbiased parties. I believe it is something we need in this country as much as any one thing, and personally I am greatly interested in it and I feel it is one of the things the INSTITUTE must take up in time, and the sooner the better.

MR. BLOOD:—This has been one of my dreams as well. The trouble comes in the case of either the real or assumed engineering quality of the different societies. There are certain of the engineering societies that have associated under the name of the Association of Engineering Societies, but the four large societies, mechanical, electric, civil and mining engineers, have hitherto kept apart. It would seem to me that in a general association of these large societies there might be a general arrangement whereby certain projects which would be impossible in a single society might be taken up in connection with others, that would be a good thing.

MR. A. L. CLOUGH:—This matter has I think been taken up by other than technical societies. If I remember rightly the American Association for the Advancement of Science at the last meeting dwelt somewhat on this matter and it would seem to me that the scientific societies could throw their whole influence together to that end.

MR. STEINMETZ:—From this discussion it seems to me that we are not quite ready to take up this matter. Undoubtedly if the Government could be induced to interest itself therein, the standard given by the Government might be expected to be accepted as authority, and universally followed.

PROF. W. M. STINE:—A suggestion is made in the paper regarding the adoption of the incandescent lamp as a primary standard of light. In 1885 a committee of the British Association[1] brought forward in a formal resolution that "a unit of light is obtained from a straight carbon filament at right angles to the middle of the filament, when the resistance of the filament is one-half of its resistance at 0° Cent. and when it consumes 10^9 C.G.S. units of electrical energy." It was further proposed[2] to make a large number of subjective experiments on human eyes to obtain a coefficient for the expression of the illumination from the standard lamps by the change in the resistance of the filament.

In such manner when comparing sources of illumination, the standard filament might be adjusted until the spectrum curve of

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1. *Brit. Asso. Rep.*, 1885, page 63.
2. Ref. Cit. page 83.

its radiation should be that of the light compared. Then the total heat and light radiations of the illuminating source and the standard lamp could be compared at equal distances by means of a thermopile. From the known radiant properties of the standard lamp, established by researches on the standard filament, the compound light could be completely defined.

Abney[1] had already proposed the definition of a standard of white light by experimentally establishing formulas which should connect the radiation from the filament with the energy current electromotive force, resistance and temperature quantities. He proposed the adoption of a standard spectrum for the comparison of the quality of lights, the quantity to be determined photometrically.

The dimensions of the carbon filament and the electrical quantities involved in its operation are all capable of ready and exact determination. It would thus seem admirably adapted for a standard light or an absolute photometry standard, in the sense that its light radiation might be completely specified by reference to the dimensions of its filament and its temperature of operation: or that the quantity of light Q should be

$$Q=f\,(A.\ S.\ T.)$$

when the functional dimensions refer in their order, to the emissivity, the acting surface and the temperature.

Further, such a standard as was pointed out in the British Association report, would be exceedingly flexible, and not only capable of adjustment to agree in quality with the compared lights but from the continuous nature of the spectrum of carbon at a certain temperation it would conform to the requirements for normal quality of light. Certainly no source of illumination as yet proposed for a standard light has so many obvious advantages.

The failure of the incandescent light to fulfil its promise of becoming an exact standard of light has been due to a lack of reproducibility and constancy of its physical character when in operation. Though it would seem to be difficult to determine with accuracy the area of the hot radiating surface, yet doubtless this could be accomplished were it the only obstacle.

The essential difficulty results largely from the tendency of carbon to assume an allotropic form at very high temperature. It is seemingly impossible to produce homogeneous carbon filaments or to flash filament until the surface assumes known radiating qualities.

The presence of graphitic carbon greatly modifies the temperature change of resistance so that the specification of a certain change of resistance to define the temperature is not feasible. The proposition that the operating temperature shall be defined by a decrease of the resistance to one half of its value at 0° Cent. loses all certainty through the lack of homogeneity in the filament. A further difficulty is introduced by the rapid change of resistance due to hysteresis and its low progressive rise due to molecular readjustment or annealing.

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[1] *Brit. Ass. Rep.* 1883 page 422.

The variation in the emissivity, both in the same filament and between different filaments is an added uncertanity. Join to these the influence of the blackening of the chamber walls, their indefinite absorption, and the vaporization of the filament, and the causes for the failure of the promising standard are apparent. The failure, briefly, lies in an inability to establish the light emitted as a function of the dimensions and physical properties of the filament.

Even should the manufacturers by refinement of their processes attain a fair certainity and uniformity of the product, these objections would still remain in force against the lamps becoming exact scientific standards of light.

I would especially call attention to the frequent statements berating the accuracy of photometric measurements. It will not do to speak of photometry as an inexact science and to claim that different laboratories cannot get concordant results. In 1895 Liebenthal[1] showed why such results were not concordant. He showed us the value of the temperature correction and most of all, the humidity correction. Introducing these corrections, when the color of the lights compared does not materially differ, the amyl acetate or pentane standards will lead to very excellent results. Lack of precision can now justly be laid to the failure of the photometrist and not to the method of photometry.

Mr. Doane:—I made a series of investigations once, extending over several weeks, and during that time I found that carbon surfaces of the two kinds gave only two emissivities; that is all "treated" carbons which I tested gave exactly the same illumination per unit surface per watt radiated. There was absolutely no difference over wide ranges of specific resistances and specific gravities of these carbon deposits. You can make deposits from any hydrocarbon or alcohol or any carbonaceous fluid and (so far as I have investigated) the light radiated per unit surface per watt will be absolutely the same.

The manufacturing companies to-day know a great deal about making incandescent lamps to an exact candle power. They find it difficult to make lamps to an exactly predetermined constant because they can't make their resistances and their surfaces what they wish. When it comes to predicting what candle power a carbon will give at a given voltage after the lamp is made, that they can do within close limits. In the course of two or three years there will undoubtedly be presented to the world satisfactory instructions for making some kind of a primary standard. What we need now is a way of getting together practically rather than theoretically, and I hope that this discussion won't end until a plan is suggested. I hope you will suggest some scheme by which we can maintain the standard of light. The amyl-acetate standard is not interpreted by various institutions or laboratories in the same way, that is they do not give the same values, consequently we are practically without a standard to-day.

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[1] *Elektro Tech. Zeit.*, Oct. 1895.

PROF. PUFFER:—I have been very much interested in listening to the discussion and (as the result of a great many experiments) I want to suggest a correction which ought to be applied to nearly all photometers, and that is a percentage correction for the common sense of the observer. What I mean by that is shown very forcibly by a set of observations I call to mind which were taken by a so-called skilled observer who carefully set up a photometer in a whitewashed vault. The lamps he tested were unusually near the whitewashed wall and the efficiency was remarkably high. Of course that was a very rank case but you will find that the reason for a great many small discrepancies in such standards as we have is due almost entirely to utter neglect of common precautions of screening the reflected light from the lamps under comparison in the photometer itself. Probably everybody in comparing standard lights against one another would put the lamps at the opposite ends of the photometer. If they would oftener try the experiment of reversing the photometer, disk and lamps they will find much better and concordant results. I agree with a number of gentlemen who have said that the amyl-acetate standard is not a favorable one on account of its color but I believe the amyl-acetate flame when used in lamps provided with a screen having a hole of a certain size will be found satisfactory. The bulk of our experience here points very decidedly in favor of a screened flame. I agree with Mr. Doane that it would be very desirable for the INSTITUTE to go on record as saying that for the present it seems desirable to take the amyl-acetate lamp as a standard and taking a certain value of it,and using that as a standard of light for the time being and the added precaution that both be placed at the same end of the photometer.

[COMMUNICATED BY REGINALD A. FESSENDEN.]

In previous communications[1] by the writer the following were suggested:

1. A standard of radiation.
2. A standard of light (to be used as primary standard), which need not be accurate to much within one-half per cent., in view of the great difference between the eyes of different observers in regard to visibility of light of different wave-lengths.
3. A secondary standard of light, consisting of electric incandescent lamps of 16 C. P., the filaments being placed in bulbs of exceptionally good vacua and of the size used for 100 C. P.

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[1] TRANSACTIONS A. I. E. E., vol. xi., p. 110; vol. xii., p. 500; vol. xiii., p. 201

Since the publication of the above the following points have been noted: That as the yellowish green part of the spectrum seems to be the most constant in physiological effect the standard of luminous effect should be one watt of light of this wave-length.

That the blackened copper ball suggested by the writer as a standard of radiation does not give a perfectly uniform distribution of radiation; or rather, that the presence of convection currents in the air interferes with the uniform distribution of the radiation, as one part of the ball is thus rendered slightly hotter than another. This might be obviated by placing in vacua, but other difficulties are thus introduced. The solution finally adopted is that of having a plane surface of copper of large extent, blackened with platinum black to a fixed degree determined by the current passed. This method of coating with platinum black was first suggested by Lummer and Kurlbaum, and with wires placed within the copper slab of such resistance and carrying such current that .01 watt is radiated per square cm. of surface of the slab; the slab to be one meter in diameter, of circular section, one cm. in thickness and its back and sides covered with a layer of three cms. of eider down; the slab to be supported in a horizontal plane, and face downwards, and with rim around it. Under these conditions it has been ascertained from preliminary experiments on a disk of smaller size, convection currents play a small part and the radiation to a point placed 10 cms. from the face of the plate is very closely the theoretical amount.

In use with this standard the balance thermopile described by the writer in a previous communication must have its faces at right angles, like the letter L, and be reversible, as before.

As regards the acetylene standard, in view of the fact that the University has not seen fit to make any appropriations for scientific work during the last five years and the writer's personal means have been taken up in working along a different line, but little further has been accomplished. The two-jet burner described by the writer has been generally adopted in commercial acetylene lighting, with however one modification which should be avoided, *i. e*, the introduction of holes in the tips for leading in air. If the pressure is high, as recommended originally, and the tips are of platinum, or platinum iridium, preferably blackened, the tips never get hot enough to decompose the acetylene and form benzene and no smoke is produced. If it were found desirable to lose less pressure the tips might be artificially cooled, but the writer believes the high pressure best.

Prof. Goldsborough:—In order that we may bring this discussion to a focus I move you that a committee of the Institute be appointed to look into the matter of supplying standard lamps to those desiring them, these lamps being compared and standardized by the committee in such manner as it deems best and most accurate. I don't mean that the Institute shall order the committee to proceed with this work directly, but that the committee be directed to take steps toward it and by consulting with the Council determine finally what is the best plan; and that this committee be also recognized by the INSTITUTE as having in its charge and in its care the matter of the development of a laboratory for standardizing lamps and instruments. My recommendation is for the appointment of a committee which shall make its report to the Council rather than the INSTITUTE. It seems to me we can safely place the matter in the hands of a committee acting with the advice and consent of the Council.

THE PRESIDENT:—The one point to be remembered is that this is a matter of ways and means and an expenditure of time and money, and that should come from the Council unless the INSTITUTE is prepared to vote an appropriation.

PROF. GOLDSBOROUGH:—Then I should be in favor of empowering the Council to spend such funds on this matter as in its judgment is a reasonable expenditure in this connection at this time.

THE PRESIDENT:—Will you kindly crystallize your motion then and present it to the meeting?

PROF. GOLDSBOROUGH:—I move that a committee be appointed to have in its care the furtherance of the establishment of a standard laboratory for testing lamps, instruments and materials and that this committee look into the matter of standardizing incandescent lamps for the general profession, such lamps to be compared and standardized by methods which the committee shall devise. That further this committee shall make its report to the Council of the INSTITUE with which it shall advise, and the Council is authorized to promote the work of this committee financially as shall in its judgment seem best.

MR. MATTHEWS:—I second that motion, with the idea that the committee will take a little broader view of the laboratory than expressed in those words.

MR. C. W. RICE:—I would like to ask if that is in a form that you yourself say can be carried out.

THE PRESIDENT:—As I understand it, this matter will be left to the Council and this is a recommendation to the Council of the sense of the meeting; consequently it remains with the Council to say whether they can see their way to carry out the wish of the INSTITUTE.

PROF. GOLDSBOROUGH:—And that the INSTITUTE gives the Council the power to act as far as an appropriation of funds for the work is demanded.

MR. STEINMETZ:—I should however like to amend this recommendation to that extent that this committee is empowered or requested to confer with the large lamp manufacturers so as to gain and secure their experience in this line.

MR. RICE:—I should like to amend by including the *National Electric Light Association.*

Mr Doane:—And others prominently connected with the work.

[Motion adopted.]

Mr. Hammer:—I should like to call attention to the fact that Mayor Quincy who honored us with his presence the other day; delivered an address in Philadelphia during the recent Franklin celebration during which he stated that Benjamin Franklin at the time of his death had left a sum of money to the City of Boston, which has since increased I believe to four hundred thousand dollars; and at the present time the authorities are considering seriously what is the best thing to appropriate this money for; it has occurred to me that possibly in view of the manner in which the Mayor expressed himself the other day it would be well for this Committee to communicate with him, with a view that possibly this sum or at least a portion of it which has been so long kept intact and which I understand the authorities are now ready to appropriate to some purpose, might be appropriated in this direction and it seems to me it would be a most excellent way of perpetuating Franklin's name and fame.

Mr. Blood:—The object of Franklin was to promote industrial or trade schools for the city of Boston, although it is not definitely stated. He gave it to certain clergymen and the Selectmen of the City of Boston as trustees and there is great trouble here now to determine who should act as trustees. The Aldermen claim they are descendants of the selectmen, and the Council claim *they* are, and the Aldermen and Councilmen so far outnumber the three prominent divines that the divines do not have the representation that was intended.

The President:—If there is no further discussion upon this paper we will now pass to the next matter.

Mr. Hammer:—I move that the Chair appoint a committee to prepare a set of resolutions expressive of the appreciation of the Institute of the many courtesies that have been extended to them.

The President:—It gives me great pleasure to put that motion, seeing that we have received many courtesies through the exertions of the Local Committee.

[Motion adopted.]

The President:—The Chair will appoint a committee of three to draft suitable resolutions to be presented before the close of this meeting, that committee to consist of Mr. W. J. Hammer, Mr. H. Ward Leonard and Mr. W. D. Weaver.

Mr. Alton D. Adams then presented the following paper on "Motor Speed Regulation."

A paper presented at the Sixteenth General Meeting of the American Institute of Electrical Engineers, Boston, June 28th, 1899, President Kennelly in the Chair.

MOTOR SPEED REGULATION.

ALTON D. ADAMS.

While great advances have been made in the application of alternating machinery, the distribution of power by direct currents was probably never increasing faster than to-day.

Tens of thousands of horse power are now delivered by direct current motors, and a large part of this work is done at variable speed.

In spite of this extended use, the direct current motor as commonly used for variable speed is by far the least efficient link between the central station engine and the consumer's machinery.

This lack of efficiency is by no means inherent in the motor but results from the common method of speed regulation by the use of a variable resistance in the armature circuit.

As the case now stands, the central station furnishes about 80 per cent. of the power developed by its engines to consumers. Manufacturers provide motors of from 80 to 90 per cent. efficiency at full speed and load, and the user in order to regulate the speed to his requirements commonly employs a method of regulation which reduces the efficiency of his motor to 50, 25 or even 10 per cent.

Consider for example a motor having an efficiency of 86 per cent. at full load and speed, with losses of 3 per cent. in armature windings, 3 per cent. in shunt magnet winding and 8 per cent. local currents, friction and hysteresis.

Let this motor be loaded to full and constant armature current and then regulated for variable speed by a resistance in the armature circuit.

As the energy entering the armature circuit is 97 per cent. of that drawn from the line, and the loss in armature winding is 3 per cent. of the total, this winding loss is .03 ÷ .97. = 3.09 per cent. of the energy entering the armature circuit.

The pressure required to force the full current through the armature resistance is therefore 3.09 per cent. of the line pressure.

The counter electromotive force of the armature at full speed will be 100—3.09 = 96.91 per cent. of the line pressure, and at one-fourth speed the counter electromotive force will be 96.94 ÷ 4 = 24.22 per cent. of the line pressure.

At quarter speed, then, the rheostat in armature circuit must consume 100—(3.09 + 24.22) = 72.69 of the line pressure, and as 97 per cent. of the total energy is delivered to this circuit the rheostat in this case consumes 97 × .7269 = 70.5 per cent. of the energy taken from the line.

Assuming that the losses from local currents, hysteresis and friction vary directly with the speed, the losses internal to the motor at one-quarter speed, become (8 ÷ 4) + 3 + 3 = 8 per cent., and the total losses in motor and regulator became 70.5 + 8 = 78.5 per. cent., thus giving the combination an efficiency of 100 — 78.5 = 21.5 per cent. at one-quarter speed.

At less than constant torque and armature current, the combined efficiency will evidently be lower than above figures.

As there are well known methods of speed regulation which involve only the small losses internal to motors, whatever the speed, it seems that a stronger effort on the part of manufacturers to introduce machines with efficient means of speed regulation, would benefit all concerned.

Two practical methods of motor speed regulation are the variation of magnet strength, and the variation in arrangement of armature conductors as to each other.

A rheostat in the shunt magnet winding, gives any desired speed above the minimum, with constant armature capacity, and two or more separate windings and commutators on the armature, give two, three or four times the minimum speed, with corresponding increase in armature current capacity.

The work required of variable speed motors is of three kinds, namely, constant whatever the speed, varying directly as the speed, and varying directly as same power of the speed.

These three classes of work are illustrated by machine tools, which require, when in use, nearly constant power for all speeds, by shafting which consumes power nearly as its speed, and by centrifugal fans whose driving power varies as the cube of their speeds, friction aside.

As constant power at all speeds involves a constant armature current, it is well provided for by a variable magnet strength to correspond with all desired speeds, but special proportions are necessary to avoid sparking with a constant armature reaction and weak magnets at the higher speeds.

When the required power and consequently armature current varies directly with the speed, the armature reaction, winding loss and magnet strength may be held constant, and the desired speeds secured through two or more separate windings and commutators, provided a reduction of the maximum speed to one half or one quarter is sufficient.

If the power and armature current must vary as the cube or other high power of the speed, it is well to combine the method of multiple armature windings with the variation of magnet strength, as the variation of armature conductors which gives the desired speed does not in this case afford proper current capacity.

It is certain that motors constructed far above methods of speed regulation will cost more than those of ordinary type, whose speed can only be regulated by a resistance in the armature circuit.

It is equally certain that the saving of energy effected by the more efficient means of speed regulation will soon offset its increased cost in any given case.

Efficient means of regulation in variable speed motors will benefit manufacturers through the sale of more valuable machines, the consumer by a reduction in power bills and central stations and manufacturers alike in the increased use that a large reduction in the cost of operation will produce.

Attention is called to these facts, not because they are new, but because their importance entitles them to more consideration than they have received.

Discussion.

Mr. John B. Blood:—In this connection I think most every engineer appreciates this paper, and in view of the fact that there are so many methods in the market it is bound not to go without appreciation. It is generally found I think that the extra complication for small apparatus will not pay for the economy gained, on account of the fact that the total watt hours of small apparatus do not figure to any large extent. In this connection I will mention that there is one method that has not been brought out, namely the method of variable electromotive forces.

There are four methods used to do this work; the methods of auxiliary machines like those of Leonard and Burke, and there is one other which has no name; the method of varying the polarity of which there are a great many, Professor Anthony has one, and Bauck in Germany and Rechniewsky in France and a number of others. Then the method of two commutators, which has not been used much in this country, that gives two speeds, and in Germany they have combined that with the variable field to get seven speeds, three in parallel p sition and four in series position. This method is used in connection with water pumping to quite an extent, and there usually the two speeds are sufficient without the variable field. Then the last method as I mentioned above is the method of variable electromotive forces. This has been used to some extent in this country and by Mather and Platt in their printing presses for cloth. They use three different voltages, combine them in different ways, which gives them with variation in the field strength as they claim, fourteen different speeds. In this printing machinery the actual speed is of more importance than the efficiency; and efficiency would not be considered if speed could not be obtained. Mather and Platt I understand obtained good results with their apparatus. There are some printing machinery plants used in this country with the variable voltage method. It is used in Cocheco. The Leonard system is used at Pawtucket. The auxiliary machine system where the voltage is varied from zero to the maximum gives the most desirable range for cloth printing machinery.

There is one point in connection with this variable speed, that is the torque of the motor is proportionate to the weight, and as with a given motor you cannot change design so as to get more than a certain torque, therefore the power at the maximum speed is the maximum power. So that in a combination where power is constant, independent of speed, you always have as a maximum power that obtained at the maximum speed. This is a point which is encountered in connection with machine tools, where the speed in cutting is constant, the power is constant and the torque will be proportionate to how far the tool was from the center.

Prof. C. A. Adams:—The shunt motor with variable field gives constant power at all speeds and is the therefore natural type for the driving of machine tools, the only objection being the narrow range of speeds that can be obtained without destroying the commutation. We have in our laboratory a 30″ motor-driven lathe, in which the above method is employed, a wide range of speeds being obtained by means of Ryan balancing coils. This motor operates sparklessly at all speeds and at all loads. It was designed by students.

[In the absence of the authors, the following paper on "Electricity in Coal Mining" was read by Prof. D. C. Jackson.].

N. B.—*Proceedings of Boston meeting to be continued in October issue.*

APPLICATIONS FOR MEMBERSHIP.

Applications have been received from the following candidates for Associate Membership, which will be acted upon by Council at its meeting, November 22nd, 1899.

No. of Application.	
1569	Clinton Eugene Whitney, New York City.
1570	Arthur James Hanson, Sydney, N. S. W.
1571	Geo. H. Gibson, New York City.
1572	Frank George Baum, San Francisco, Cala.
1573	Chas. B. Raub, New London, Conn.
1574	Charles William Pendell, Boston, Mass.
1575	Hewlett Scudder, Jr., New York City.
1576	C. M. Wilkes, Chicago, Ill.
1577	W. O. Layman, St. Louis, Mo.
1578	Ferdinand Schwedtmann, St. Louis, Mo.

Any member objecting to the election of any of the above candidates should notify the Secretary to that effect prior to November 22nd, 1899.

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Contents of Vol. XVI, 1899, far as Published.

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